THE OHIO

MEDICAL AND SURGICAL JOURNAL.

Vol. 11. Columbus, March 1, 1859. No. 4.

PART FIRST.

ORIGINAL COMMUNICATIONS.

The Cole Case. Reported for the Journal by H. M. MIXER, M.D. Attorneys for the State, A. G. RIDDLE, WM. O. FORREST, A. S. HALL, Prosecuting Attorney for Ashtabula county, and H. K. SMITH, Prosecuting Attorney for Geauga county. Attorneys for the Prisoner, A. H. THRASHER, R. P. RANNEY and L. S. SHERMAN.

Mrs. Adelia Cole died at Bainbridge, Geauga county, Sept. 9, 1857. Former difficulties with her husband, Hiram Cole, his adulterous connection for a length of time with another woman, and circumstances immediately connected with her death, led to the suspicion that it was produced by poison, administered by him, and he was accordingly put upon trial.

It appeared in evidence, that Mrs. Cole swallowed one and a half drachms of laudanum, saturated with arsenious acid, and which, with what floated in suspension, probably amounted to from three to eight grains.

She at once retired to bed, and was very soon asleep. In about three-fourths of an hour she awoke in great agony. Her husband and the family, where they were tarrying, being aroused, found her in spasms.

The indictment charged, in substance, that he procured her death by administering arsenic and laudanum, and subsequently arsenic in coffee and also in epsom salts.

Upon this indictment he was twice tried—once in Chardon, Geauga county, at which time the jury failed to agree, and again at Jefferson, Ashtabula county, Ohio, when he was acquited.

This report is confined to the more important parts of the testimony adduced in the last trial.

Mr. Hall, on behalf of the State, expected to prove essentially as follows: That Mrs. Cole came to her death by poisoning, administered by her husband, Hiram Cole; that she had taken arsenic a few days before her death; that she died with many of the symptoms of poisoning by strychnia; that a mixture of laudanum and arsenic was substituted by him for a medicine used by his wife; that a teaspoonfull of this preparation, taken at 9 P. M., September 8, 1857, produced acute suffering within an hour; that from the use of medicine administered she became better, so as at 4 A. M. to be out of danger; that the patient continued to improve until about noon of the following day, when her husband gave her a cup of coffee, when the symptoms reappeared, and that between 3 and 4 o'clock P. M., he gave her a dose of epsom salts, when within 30 or 40 minutes tetanic spasms appeared, the sixth or seventh of which proved fatal.

Mr. Thrasher, in behalf of the defence expected to prove that Hiram Cole, being the proprietor of a livery stable, was accustomed to keep a mixture of laudanum and arsenic, which he gave to his horses for worms and botts; that Mrs. Cole took a drachm and a half of medicine, as she supposed, from a preparation that she used to prevent conception, but which was by mistake this mixture; that the quantity of laudanum taken was sufficient to produce death; that it contained an indefinite quantity of arsenic, and that the symptoms were those of poisoning by arsenic, or arsenic and opium.

Dr. D. Shephard testifies—At midnight the symptoms were those of poisoning by opium. Offering her coffee she cramped in spasms, and threw back her head; came out of this when he gave her coffee rapidly. Giving her coffee aconite and camphor the spasms grew lighter; at the end of four hours she sat up in bed and conversed cheerfully. Head perspiring and hot, countenance flushed, spasms came suddenly, were intense, involved voluntary muscles, lasting near a minute, and apparently started by external stimulants. No tenderness at stomach, vomiting or purging; but complained of burning sensation in mouth, throat, and stomach. Spasms came on every few minutes, during which her limbs were straight, head thrown back and a little to the left side—jaws shut. Was perhaps incohe-

rent once. Was very thirsty. Mrs. Cole was peculiarly susceptible to the effects of medicine, especially opium. Was of nervo sanguine temperament. At 4 A. M., the symptoms had so far subsided that the Doctor left, supposing the patient out of danger.

Mrs. A. Briggs testified, essentially corroborating the above. In spasms Mrs. C. stood upon her head and heels. Requested husband to bear heavily on stomach. Complained of back. Moving clothes started spasms. Left at 3 A. M. Returned at 5 P. M., of September 9. Spasms were near together and more severe than on the night before. Died in a spasm. She was stiff after death—both hands shut and arms stiff. Eyes open and prominent. *Toes drawn up.* Could not straighten her arms. She was stiff, and we had to almost stand her up to get her through the door at the foot of the stairs. Her jaws were firm.

Cross-examined.—Don't know as noise disturbed her, but moving the clothes did. The night she died, when we went in, she said, "softly, softly," as if noise disturbed her. *Toes turned up.* Feet were not against the foot-board the night she died.

[We have been under the necessity of so condensing the above as to relieve Dr. Mixer entirely from the responsibility of any imperfections that may characterize it. Important and able testimony from several physicians, as that of Drs. Curtis, Vincent, Harmon, Sweeney, Bennett and Fifield, we are obliged entirely to omit. That of Prof. Cassels, covering the entire ground taken by the State, we give in full, omitting again that elicited on the cross-examination of Dr. Hubbard.—EDITOR. H.]

Dr. L. C. Hubbard, examined by Mr. Riddle.—I am a physician; attended lectures at Willoughby and at New York city; practiced thirteen years, and reside at Ashtabula. Have given the subject of poisons no particular attention. Arsenic, one of the most acrid, excites irritation of animal tissues, partly on account of its caustic nature. It acts also upon the nervous system at large, producing manifest symptoms of dryness ad burning in the mouth, throat and stomach. Pressure upon the stomach would be unpleasant after pain had set in, and vomiting and diarrhea would soon follow. In solution symptoms would begin in from a few minutes to perhaps half an hour, and constitutional symptoms would begin soon after vomiting. Pulse would be rapid and small—countenance become pinched, with an expression of distress, and skin cold and clammy. The

effect of arsenic is depressive. Should expect the vomiting would be severe. There would be a good deal of febrile action. After the system had become saturated with it the pulse would be small. In the last stages the nervous symptoms might become prominent. There would be depression and, in some cases, lockjaw and convulsions. Intellect usually remains quite clear. In last stages may be wandering of intellect and coma. Death would commence in the circulation—in the heart; then in the nervous system—the brain; and then the lungs, finishing the patient. The convulsions in arsenical poisoning would mostly seize upon the limbs. Well defined spasms are never, to my knowledge, the first symptom. Intermission of vomiting and purging may take place, but the constitutional symptoms would remain. The irritation may be suspended. Think absorption itself could not cease for five or six hours. If arsenious acid were given should expect a few grains would be formed in the stomach. If fatal dose of laudanum were given should expect intoxication—then stupor, slowness of pulse and breathing. Would affect the nervous system first, then the pulse and breathing. Think opium and arsenic, each in fatal dose, could be readily distinguished by symptoms. Were 1 1-2 drachms of laudanum, containing in solution 1-7 or 1-8 of a grain of arsenic, administered should expect opium symptoms. So with 1-2 grain of arsenic in same quantity of laudanum—and even with one grain of arsenic should expect the laudanum would kill the patient.

Symptoms with strychnine poison are first some agitation of the nervous system and quickness of pulse; after these premonitory symptoms general convulsions would follow—all the limbs would be agitated—breathing difficult and suspended at hight of spasm—eyes glaring wide open and lockjaw—strongest muscles would prevail—arms would be crossed—fingers clinched—body bent backwards—abdomen thrown out. Should expect remission of spasm in animals—almost intermission. Have seen three or four dogs and some frogs killed with it. Should suppose the symptoms would be analagous to human beings. Intellect usually remains clear to the last—sensitiveness to noise or rustling of clothes—this last is noticeable during the intervals. Could not, in animals, determine the state of reason during the hight of spasm. In some cases there would be vomiting. In most active poisons thirst is produced. Strychnine affects first the voluntary and afterwards the involuntary muscles. In spasms sufferer would rest prominently on head and shoulders

and lower part of the body. In solution would act in from three to fifteen minutes. In a minimum fatal dose should expect death within ten hours after spasms commenced. Should expect spasms sooner in case of a larger dose in solution. Contents of stomach would modify action some; full stomach would retard, while much gastric juice would facilitate action. Think the symptoms are well defined; a wide contrast between it and arsenic. Could determine from symptoms pretty accurately what the poison was. Should expect to find the body rigid after strychnine—frogs were invariably so—don't recollect in regard to the dogs. In arsenic poisoning I should expect a flaccidity. My opinion is that when death occurs during a convulsion, from any cause rigidity remains. If a person, upon going to bed at 9 o'clock P. M., takes a dose of some kind of poison, and in forty-five minutes is seized with well-defined tetanic spasms—these spasms lasting two or three hours at intervals, with some thirst and slight uneasiness of throat and stomach, and then gradually pass off, the patient all the while remaining conscious—I should say that strychnine was the poison taken. And suppose this person to be a delicate female, aged 23, and it is known that she took at the time mentioned one and a half drachms of laudanum, with one-sixth to one-eighth of a grain of arsenic in solution, I should still think that the laudanum and arsenic did not produce the symptoms, but that they were due to strychnine.

Arsenic sometimes produces trembling, spasms and lockjaw, but never, so far as I know, general tetanic spasm. Indeed, I object to the use of the term "tetanic spasm," as applied to the affection resulting either from arsenic or strychnine. Tetanus is tonic spasm—spasm not alternating with relaxation—and the spasm of both arsenic and strychnine is clonic—that is spasms alternating with relaxation. Still I think there is a wide contrast between the spasms of arsenic and strychnine—though neither of them are truly "tetanic." If the female I have supposed continue comfortable from the time the spasms passed off the first night until 3 o'clock P. M., of the next day, at which time, after taking a cup of coffee, she expresses some apprehension of a return of spasms—at 4 o'clock 30 minutes takes a dose of epsom salts, (say a teaspoonful,) and at 5 o'clock is taken with the same kind of spasm as those from which she suffered the night before, and in the sixth or seventh spasm died, I should say the death was attributable to some violent disease suddenly supervening, or to poison. And, if we exclude the supposition of

disease, I should suspect strychnine. I do not know of any other poison capable of producing such symptoms. Should not think they could be caused by a teaspoonful of laudanum and one-eighth of a grain of arsenic, taken the night before.

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Prof. J. L. Cassel, examined by Mr. Riddle.—I am Professor of Chemistry in the Cleveland Medical College, and have been teaching chemistry for 23 years. Received my medical education at Fairfield, Herkimer county, N. Y. Have practiced medicine some. I received certain drugs from Mr. Smith, of Chardon, for examination and analysis: One labelled "Vest Pocket;" second, "Stand Drawer;" third, "Horse Medicine;" fourth, "Mrs. Cole's Medicine;" fifth, "Epsom Salts." The first and second I found to be arsenious acid; third to be laudanum and arsenic, some in solution and some as a sediment; from taste I found the fourth to contain bloodroot, and by test some laudanum; the fifth was epsom salts. I examined the entire stomach, and its contents, of Mrs. Cole, received from Mr. Sprague. I tested for arsenic, corrosive sublimate, strychnine and oxalic acid. Thought I detected slight traces of strychnine, but nothing upon which I could rely. Wish to be understood as saying that I found none. Found no poison in the stomach. Cannot say whether the stomach was reddened by previous inflammation or by infiltration of blood after death. The stomach submitted was some red. Think arsenic would show irritation of coats of stomach. Finding no arsenic in the stomach would not be positive evidence that the patient did not die from that poison. Some time afterward I examined the liver and portions of the intestinal canal. Found small quantity of arsenic in the liver; but not more than I should expect from previously taking arsenical preparations in medicinal doses. Arsenic is found in largest quantities in the glands—as liver, kidneys and spleen. As some also goes into the circulation, it might be found in other parts of the body. Strychnine is classed among narcotic and irritants, and is also called a narcotico-irritant. It is almost impossible to mention any one symptom that attends throughout all cases of arsenical poisoning. Arsenic is a mineral poison. Taken in solution symptoms are heat and burning in stomach, throat and intestines—bloody stools, &c. Would not endure pressure over the region of the stomach. Mind is clear, up to the time of convulsions. Arsenic acts through the medium of the circulation. No matter how applied it acts upon the stomach. Convulsions generally arise in the latter stages.

If the arsenic acts as an irritant upon the stomach see no reason why we may not get convulsions. Would come on slowly, affecting voluntary muscles, afterwards the involuntary. Think it is not to be expected that convulsions should occur as a first symptom. There may be interruption in the symptoms of arsenical poisoning. Patient may express himself free from pain, yet hidden operations be still going on. Do not think a return would be expected after a lapse of twelve or fifteen hours. Cannot arrest the progress of arsenical poison after absorption has taken place. Should think it would be impossible for a return of same symptoms after five or six hours. Should think if arsenic was taken in less than poisonous dose it would be completely eliminated from the system in two weeks. Laudanum is a narcotic poison. The effects are first exciting, afterwards sedative. In nervous persons the circulation is rapid—hence would take effect sooner, as it would be sooner carried through the system. One drachm would be a large medicinal dose. Should think 1 1-2 drachms might be a fatal dose to a sensitive female. By experiment found that one drachm of laudanum would not dissolve quite one-tenth of a grain of arsenic. The effect of arsenic is very uncertain. Cannot tell how much arsenic one would turn out from a bottle containing arsenic and laudanum mechanically mixed in such proportions as horse medicine, and from like bottle. Think that with one grain of arsenic mixed with one drachm of laudanum, the narcotic symptoms would predominate, and arsenic would go on, and hidden by narcotic symptoms, might produce death. Tried experiments with horse medicine. Gave two cats a teaspoonful of the horse medicine without shaking up. Died with narcotic symptoms. No signs of pain. Strychnine is an extract from nux vomica. Strychnine is an alkaloid. Do not know what a minimum fatal dose of strychnine would be. In one case a patient took four grains. In another two were put into "Hoffman's Bitters." These were fatal cases, and I found strychnine in the stomachs of all of them. It is supposed to be taken into the circulation, affecting spinal chord and thence the muscles. Among the first symptoms of strychnine are convulsive twitchings of the lower extremities. Afterwards general convulsions.

There is sometimes heat in the region of the stomach and also thirst. Patients usually die during a spasm—not always. They are sensitive to surrounding noises and movements—mind generally clear after tetanus is established. I do not know as it differs in any respect from tetanus produced by wound or other cause. Arsenical

poison usually has its convulsions near the last. Think the symptoms attending strychnine and arsenical poison can be separately identified. I should suspect from certain trains of symptoms, certain agents as their cause. I think I could detect strychnine symptoms from those of any other poison. If I could arrive at the history of the symptoms in minutia from the first, I could tell. Could distinguish between laudanum, arsenic and strychnine under like circumstances. Strychnine is a compound body, and may be decomposed in the tissues of the body before death. No strychnine has ever been found to my knowledge in the animal tissues when given in minimum doses. Have often found strychnine in the stomach—suppose it was lodged upon the mucous membrane. Suppose strychnine is decomposed by the tissues, because it is not poisonous to other animals. Have tried Gerdwood & Rogers' test, when I supposed there was strychnine, and found none. I think in the event of the administration of six grs. of arsenic, partly in solution and partly in suspension, and death occurs in 20 hours—arsenic would be found in the stomach.

The State here supposed a number of cases, and asked opinion of witness, as follows:

Suppose a patient—a female, 22 or 23 years old, of slender yet ordinary health, with more than usual predisposition to medicinal influences—at 3 1-2 hours after her usual supper—takes a teaspoonful and a-half of some unknown liquid and retires—that within 45 minutes she is attacked by well-defined tetanic spasms involving the whole voluntary muscular system—coming on suddenly—continuing a few minutes—returning after short regular intervals, and succeeding each other in rapid succession, with increased violence, for 2 1-2 hours—accompanied by extreme sensitility of surface of body—intolerance of sounds and slight movements of the bed clothes, and at one time thrown into violent spasms by a drop of water falling upon her neck—these spasms accompanied by difficult respiration—patient complaining of burning sensation in throat and fauces, together with an absence of nausea—vomiting or purging—the patient insisting upon being held down by the application of force over the chest and abdomen, and these occurring in the absence of tetanus either idiopathic or traumatic—what in your opinion was the cause?

I know of no agent capable of producing that condition of things except strychnine or some of its compounds.

Suppose further the case to have reached its maximum at 12 o'clock and from that time the symptoms gradually abate—the spasms are

less frequent and less severe until at 3 o'clock they entirely cease—with lessened sensibility—no complaint of burning in throat or fauces—no thirst, and from that period patient continues to improve until 6 o'clock, at which time she arises, has her bed changed—complains of dizziness of the head while standing—converses cheerfully with her attendants and has a passage of urine at 7, partakes slightly of food with relish, a portion of which is soon vomited—from 7 to 9 continued improvement is made, at which time all indications of sensitiveness have passed away—the patient is cheerful—converses with visitors and complains only of slight dizziness of the head while standing—with a bad taste in the mouth—with some soreness of the muscles—at eleven A. M. falls into a sleep which is undisturbed, and from which, in 1-2 an hour she awakes naturally—at a little past 12 she partakes of food with a relish, a part of which is vomited, and she has another passage of urine—the patient progresses until 1 1-2 o'clock P. M. with constant improvement, what do you say to the probability or possibility of a return and with increased violence, from the dose already named as taken the night before?

I do not see upon what principle it can be possible.

Suppose that the before-named dose contained 1 1-2 drachms of laudanum with a gr. of arsenic partly in solution and partly in suspension, would that account for the previous symptoms and the cause?

I think not. Suppose the quantity of arsenic was increased to 4 grs. would you expect the foregoing result? I do not see how they could produce it. Suppose the above case to go on till 4 1-2 o'clock afternoon, at which time a teaspoonful of epsom salts is taken in solution—what effect would that produce? I would not expect any unless a slight laxative effect. Suppose that within 45 minutes after taking the salts the patient should be seized suddenly with tetanic spasms in all respects similar to the first named, only they are more violent and follow each other with greater rapidity, until within an hour after their appearance the patient dies in a paroxysm—that their course has been accompanied with difficulty of breathing—the extreme sensitiveness before-mentioned—slighter thirst—a distorted countenance and glaring eyes—to what in the absence of tetanus traumatic or idiopathic would you attribute this death? I know of no agent that could produce it but strychnine or its compounds. In what way would that opinion be modified by the following facts? At death the body is left with the fingers nearly closed—toes turned

up—eyes prominent and staring—lips everted—the body permanently rigid, which remains till decomposition—that several days before death the patient took a large dose of arsenic producing well-defined arsenical symptoms—that after death chemical tests detected no poison in the stomach, and very slight indications only of arsenic in the liver. Answer. I do not know as those facts would modify my opinion at all.

Cross-ex'd by Mr. Thrasher—I have not practiced medicine for twenty years. Have never witnessed a fatal case of poisoning of any kind. Derive my knowledge of poisons and their effects from the books. In my experiments I put strychnine into laudanum and with some difficulty found my strychnine again. I then tested the "horse medicine" and found no strychnine, nor was there strychnine in any of the medicines or material submitted to me for analysis.

Arsenic is undoubtedly an antiseptic agent. Generally find the bodies preserved several months in cases of arsenical poisonings. I think laudanum would accelarate the absorption of arsenic. Strychnine produces death in my opinion by asphyxia. Do not know the cause of heat. There are many varying theories upon the subject. I incline to the adoption of Leibig's theory as to the cause of animal heat. If arsenic were easily found in 1-2 lb. of the muscle of the thigh of a dead human body, and morphine in other parts of the same body, and it were known that the person had taken before death a dose of arsenic and laudanum, I should readily conclude that a large dose had been taken and death produced by it. Think that coffee would accelerate the absorption of arsenic. The books give cases where large doses of arsenic have been taken and have not operated for a long time. In one the arsenic was found enveloped in a mucous cyst, which prevented absorption. Such a thing is barely possible, for when the arsenic was lodged there would be no vitality of the stomach and the power of absorption would be lost in that part, so that even if you could suppose the cyst to be removed, still no absorption would take place. Do not think the arsenic would be absorbed by the other portions of the stomach. If the cyst were destroyed by the gastric juice the poison might possibly act, but do not see how the gastric juice could destroy it. I hardly think the cyst would be removed. Do not see how it could be. I have not examined Taylor on this point. I think he theorized just as I am now theorizing. It is only theory. They are matters about which I do not know and am ready to acknowledge I do not believe any one else does.

The evolution of heat is not always the result of condensation. In slaking lime the water becomes solid and heat is evolved but it is not always the case that heat is the result of condensation. Heat is evolved sometimes by condensation and sometimes when there is expansion—as in the explosion of gun powder. It is allowed there is such an agent as heat, but we do not know all the laws which govern its evolution. In a disease that shuts off from the lungs a portion of the atmosphere, I should not be able to account for unusual heat in the body. There is always an undue amount of heat where there is great muscular contraction. Even on Leibig's theory there may be oxygen enough in the body to produce heat.

[Court here adjourned till 8 1-2 o'clock to-morrow morning.]

SEVENTH DAY, TUESDAY, NOVEMBER 23.

Prof. J. L. Cassel's cr ss-exam nation continued by Mr Thrasher. —I did testify on the former trial of this case that I was unable to account for the great heat in the body of Mrs. Cole after death on the theory of strychnine poisoning—except upon a supposition of artificial combustion, and that I doubted. That is my opinion now. The toes are said to have been turned up in the case of Mrs. Cole. They were turned up in the Marion County case as stated by competent medical witnesses. That was a case of strychnine poisoning in which I was a witness, and in which I found strychnine in the stomach. (This was ruled out by the Court.) I think the turning up of the toes would be very rare, as the theory is that the stronger muscles prevail. Burning in the throat and stomach and great thirst sometimes occur in strychnine poisoning.

Did not you testify on the former trial of this case that there was an absence of thirst in strychnine poisoning? I may have testified so, but I am not an "old fogy." I have progressed some since then. Question. I read from a report of your testimony as follows:—In tetanic convulsions from strychnine there is not much inclination to drink water,—rather an abhorrence of it. Is that an accurate report of what you said on that occasion? It is, and I think so yet. There is a loathing of water arising, as I think, from an inability to swallow. May not tetanic spasms be produced in 3-4 of an hour by the exhibition of a fatal dose of arsenic? I can see no reason why they may not be. Is not burning of the throat, fauces and stomach characteristic of arsenical poison? Yes. Is not great thirst indicative of arsenical poisoning? Yes, peculiarly so. Is not clear intel-

lect also indicative of arsenical poisoning! The intellect is ordinarily clear in arsenical poisoning, but I do not know that this is peculiar to arsenic. Is not pain in the back a symptom of arsenical poisoning? It is. Is not faintness a symptom of arsenical poi oning? Yes. Does not great heat attend the cases of arsenical poisoning? Arsenic irritates the mucuous membrane of the stomach, and when there is irritation there is ordinarily heat.

As a matter of fact heat attends the early stages of cases of arsenical poisoning, whereas in the later stages they are usually characterized by prostration and a diminution of caloric. Is not the diminution of heat in the latter stages peculiar to the chronic cases—those that survive several days? It is. Is not extreme sensitiveness to external impressions characteristic of *tetanus*, however produced? It is. Is it peculiar to strychnine poisoning? It is not. After tetanic spasms have supervened is it possible to determine from the spasms themselves what agent produced them? It is not. I could only form an opinion by watching the whole train of symptoms from beginning to end. Is rigidity of the body after death peculiar to strychnine? It is not. After death from strychnine the body is ordinarily rigid—at least this has been true in all the cases coming within my knowledge—but when death occurs during a spasm, however induced, I should expect the body to remain rigid. May there not be opisthotonis in tetanus caused by arsenic as well as in that caused by strychnine? I know no reason why it may not occur. May not lock-jaw also be produced by arsenic? It may. Would there not be the same difficulty in breathing in the tetanic spasms caused by arsenic as in those caused by strychnine? There would be. Is frothing at the mouth diagnostic of strychnine poisoning? It is not. It arises from respiration during spasms attended with lock-jaw. Is the stomach always found inflamed in cases of arsenical poisoning? It is not. Is vomiting always present in cases of arsenical poisoning? It is not. Is purging? It is not. Is tenderness of the stomach an invariable attendant upon arsenical poisoning? It is not. It is due to irritation, and if the poison did not irritate the stomach there would be an absence of tenderness. Is arsenic always found in the stomach when death is caused by it? It is not. Does not arsenic possess peculiar antiseptic power? I believe it does. Cases are reported where bodies have been found preserved for a great length of time. Is not suppression of the secretion of urine a symptom of arsenic poisoning? I do not know, but will thank you to state how that is. If by the Stas process the alkaloid morphia were

obtained, would not the alkaloid strychnine if present be found by the same process? Most certainly it would. If one were destroyed the other would be. Is not dizziness indicative of both opium and arsenical poisoning? It is. Is not a tendency to fall into a drowse and then start up suddenly indicative of both opium and arsenical poison? It is. To what would you attribute the glaring eye and prominent eye-ball occurring during tetanic spasms? Is it peculiar to strychnine or is it due to the spasm however induced? It is attributable to the spasm. Have you ever witnessed a fatal case of poisoning with arsenic and laudanum? I never have. Now, Prof. Cassels, it seems we agree in everything, except as to the remissions in arsenical poisoning. Let us look into that matter. You say you are dependent upon the books for your knowledge upon the subject? I am. Do not Christison, Beck, Taylor, Wharton & Stille and Dean, teach the doctrines that remissions and even intermissions have occurred? I believe they do. (Mr. Thrasher here read from authors referred to several cases in which remissions and even intermissions occurred varying in duration from a few hours to several days, and asked witness if he recognized these cases as authentic? Witness said he did.)

This closed the scientific testimony on the part of the State.

DEFENCE.—Seventh Day—Afternoon Session.

Dr. L. A. Hamilton examined by Mr. Thrasher.—I am a physician—received my medical education at the Berkshire Medical School, in Pittsfield, Massachusetts. Have practiced Medicine and Surgery about 30 years. I received a portion of liver and intestines February 15, 1858, said to be remains of Mrs. Adelia Cole, which I was reqested to test for arsenic. After preparing the material, we subjected it to several of the usual tests, all of which showed unmistakeably the presence of arsenic. My partner, Dr. H. M. Mixer, was associated with me in making the analysis. I was afterwards requested to make another examination of the body of Mrs. Cole. It was disinterred April 10, 1858—seven months after death, at the request of the prisoner for the purpose of testing for strychnine. I found it in a very good state of preservation. The viscera had nearly all been removed previously. In dissecting out the portions I removed for analysis, the muscles offered nearly or quite as much resistance to the knife as in the recent subject, and indeed (except those portions a little dried) the usual fresh color of the muscles was preserved. The process of decomposition had not commenced at all in the remains left after previous examinations. The body had re-

mained quite dry. The grave clothes had not been wet by the admission of water into the coffin. I removed one lung which I found loose in the chest, together with a small quantity of blood. I removed also the entire dorsal portion of the spinal column, and about one pound of muscle from the anterior portion of the thigh. These were placed separately in clean glass jars and sealed up for future examination. I also cut open the stockings upon both feet so as to expose the toes, and found them all turned down close upon the ball of the foot. In analyzing and testing all of the material obtained, Doctor Mixer was associated with me. We examined the lung and blood for meconic acid to verify the presence of opium. We did not detect meconic acid. Some specimens of opium do not contain it. We examined the muscle for arsenic by Marsh's process and found it in considerable quantities—fifteen or twenty mirrors were deposited on porcelain and tested to satisfy ourselves that it was arsenic and nothing else. The spine was subjected to maceration to free it from blood preparatory to the use of M. Stas' process for the detection of strychnine. This analysis was made with all possible care and no strychnine detected. We did obtain morphine in small quantity, sufficient to have destroyed the color tests for strychnine if any had been present. To determine the presence or absence of strychnine with morphine, we used a solution of tannin. With this the smallest quantity of a salt of strychnine may be precipitated when in solution with morphine. Finding morphine and no strychnine by this process is to me conclusive evidence that no strychnine could have been present in the body, as it would be impossible to procure one without the other by this process if both were contained in the material used. It was arranged beforehand that the portions of body removed at the disinterment were to be divided with the State, hence we took a large quantity. The State refused to take any portion of the remains, and we therefore used the whole in our tests.

Strychnine operates upon the spinal cord, and in fatal cases is deposited upon it or its investing membrane much more abundantly than anywhere else. On this account we selected this part for examination. The symptoms of poisoning with strychnine are, permanent difficulty of breathing, coming on soon after the poison is taken and continuing with greater or less severity until death takes place; spasms commence in the lower extremities, slight at first, but gradually increase until general convulsions, involving all the voluntary muscles, are developed. The urine was discharged

involuntarily in all the cases I have witnessed, both in males and females. During the convulsion the respiration ceases, the jaws are fixed, the tongue black, and the eyeball glaring, with dilated pupil. The pulse was irregular, and often intermittent. Of the whole number of animals to which we gave strychnine, not one could be induced to drink, although water was invariably offered to them. No unusual heat was observed, either before or after death, which usually took place during a paroxysm. In all cases, witnessed by me, the bodies were flacid at the time of death. The taste of strychnine is intensely bitter, and not easily disguised. It is absorbed and enters the circulation without undergoing the process of digestion. According to the experiments of Mr. Harley, it produces death by mingling with the blood and destroying its capacity for uniting with oxygen or parting with carbon. Whether this is, strictly speaking, correct, I am unable to say. I do not think the evidence conclusive upon the subject. In our experiments upon animals, we have without difficulty found it in the contents of the stomach, liver, blood, and spinal cord. If the death of Mrs. Cole had been produced by strychnine, a sufficient quantity must have been deposited upon the spinal cord to have enabled us to detect it. In some of our experiments upon animals, we gave arsenic, strychnine, and laudanum, or morphine. On examining for it, both alkaloids were found mingled together.

The quantity of opium, necessary to produce death, is uncertain; from 4 to 6 grains would be fatal in some cases. One and one half drachms of laudanum contain more than six grains of opium, if all the strength is extracted. Six grains of opium is an unsafe dose for a feeble person, unusually susceptible to the influence of medicine. Have seen remission of symptoms in one case of poisoning with morphine, in a young child. Spasms and convulsions are sometimes produced by opium. We found that 1½ drachms of the "horse medicine" contained, by accurate analysis, 9-16 of a grain of arsenic. The smallest fatal dose of arsenic on record, is 183-100 of a grain. In poisoning with arsenic, the symptoms commence in from half an hour to one hour, usually, but have occurred sooner, and are frequently delayed much longer. So also is the period varied at which death takes place. An average period, as stated in the books, is from six to twenty-four hours. Tetanus is produced by wounds, or more rarely occurs as an idiopathic disease; is attended

with great sensibility of nervous system. The intellect is clear. Cases of tetanic spasms are related in the books, as occurring in from one-half to three-quarters of an hour after arsenic has been administered. The symptoms produced by arsenic, and also opium, are not uniform, but will vary very much in different cases. Such also is the case with all the narcotic and narcotico-irritant poisons. We cannot rely with certainty upon symptoms alone, to determine what poison was taken.

What do you say as to 1½ drachms of laudanum, fully saturated with arsenic, taken by a female with feeble constitution, nervous, and peculiarly susceptible to the operation of medicines producing tetanic spasms in from half an hour to one hour after being taken? It would not be its usual effect, but can see no reason why such result might not follow. Where a person has taken 1½ drachms of laudanum, saturated with arsenic, under circumstances when she may have taken from 3 to 10 grains of arsenic besides that in solution in the laudanum, and there follows tetanic spasms, a remission of from six to twelve hours, a return of the spasms and death in twenty-one hours from the time of taking it, and, after death, arsenic and opium are found in the body, and no other poison, what do you say was the cause of death? It is only reasonable to conclude that death was caused by that dose. When death has been caused by arsenic, are the stomach and intestines always found inflamed? No.

In cases of death by arsenic, is arsenic always found in the stomach? No. Are the symptoms any indication of the quantity taken, when a fatal dose has been given? That is, do they indicate whether the quantity was large or small? No. What do you say of intermissions and remissions in cases of arsenical poisoning? Such cases are reported in the books too frequently to admit of doubt about their occurrence. What do you say as to the probability or possibility of a female of feeble health, nervous constitution, and peculiarly susceptible to the effects of poison, having taken 1½ drachm of laudanum, with from 2 to 6 or 8 grains of arsenic, experiencing the following symptoms: Tetanic convulsions in three quarters of an hour after poison taken, which continued more or less severe for from four to six hours, with remission of more or less quiet of from ten to twelve hours, with a return of the convulsions and other distressing symptoms, and death, in twenty-one hours. Might or might it not occur? It might occur. What treatment ought a patient

receive, after having taken into the stomach 1½ drachms of laudanum, and a fatal dose of arsenic? Copious draughts of mild fluid, and followed with active emetics until the stomach is emptied of poison, followed by cathartic of castor oil, or other similar cathartic medicine. What influence would strong coffee have upon such a dose of poison? No influence, as an antidote. It would only act as a diluent. On what structure does arsenic act? It acts upon the mucous membrane and nervous system. After a fatal dose of arsenic and laudanum has been taken, and a remission occurred, what effect would a small dose of epsom salts, containing the usual impurities—either chloride of lime, soda, or magnesia—have upon the patient, probably? No important effect would be produced, unless some chemical change was effected. If, by the action of chlorine (liberated from the chloride) in the stomach, oxygen should be set free and brought into contact with arsenious acid, it might convert it into arsenious acid, which is a much more virulent poison. That such changes may take place in the stomach, is an established doctrine with the profession. Indeed, every nurse understands that salted food must not be given to patients taking calomel, lest the chlorine of the salt convert the calomel into corrosive sublimate.

Possibly the conversion, in some manner, of arsenious into arsenic acid may account for a return of symptoms in an aggravated form, after an intermission has occurred in arsenical poisoning.

Court adjourned.

EIGHTH DAY, WEDNESDAY, NOVEMBER 24.

Dr. Hamilton, cross-examined by Mr. Riddle.—From teaspoonful of epsom salts should not expect much impurity. Should expect in twenty hours the arsenic might have been all absorbed, and so passed from the stomach, and from my reading should expect to find it most abundantly in the liver. Should expect ordinarily to find the stomach inflamed. Till since the last trial I had no experience in strychnine poisoning. I have made the experiments with reference to this case, and have experimented upon six animals. In testing for strychnine I used the formula of Stas. In two cases we examined the stomach, and in both found strychnine. One of the animals we poisoned with it—the other we suspected had taken it. In no one animal did we examine the blood, liver and stomach—but we examined all these in different animals, and in every instance

found the poison. Think it can be found in any case of death from strychnine, when taken into the stomach. If injected into a vein think so small a quantity may prove fatal as to escape detection. Think I can find it if taken into the stomach, and if I did not find it I should state with confidence that death did not result from it, or that there was some error in my process. Think the color tests would detect the presence of the $\frac{1}{50000}$ part of a grain. I see no reason why it may not be eliminated from animal matter as well as from the nux-vomica. The animals upon which I experimented were those I had myself poisoned with strychnine, except the one before mentioned. The difficulty of breathing is caused, I think, by a change in the blood; yet it may be from want of nervous influences. Difficulty of breathing continues until death.

If strychnine should be decomposed in the system it would be no longer strychnine. I think, however, it is not decomposed. Think with good materials, in skillful hands and with care, there should not be a failure. I mean in the human body. If failure occurred I should look for some error in the process. I attribute the failure to find strychnine in the human body to the presence of morphine, or some other medicinal agent in the stomach or system. I think when arsenic and laudanum are given together, the arsenic would counteract some of the influence of the opium, so that a larger dose of laudanum would be required to prove fatal than when laudanum is used alone. Think the effect of the arsenic might be covered up measurably by the laudanum. In most cases the effect of laudanum would pass off in ten or twelve hours; yet it might continue much longer. Should expect the symptoms of the arsenic would be modified while the effect of the laudanum was felt. The first effect of the laudanum would be stimulating, and there might go along with it the irritating effects of the arsenic. I reason from theory simply. In traumatic tetanus laudanum, in large dose, would be a remedy.

Re-examined by Mr. Thrasher.—When the stimulating effects of a large dose of laudanum are passing off, I should commonly expect narcotic symptoms—though it would influence different persons differently. In some it produces sleeplessness—in others a sense of drowsiness, but at the same time an inability to sleep. Such persons would appear to sleep and then start up suddenly.

To what do you attribute the failure to find strychnine (if present) in the body of Cook, in the "Palmer case"? If that death were caused by strychnine the presence of morphine, which it appears

Cook had taken just previous to death, is sufficient to account for the failure to detect the poison by the color tests, upon which Taylor relied.

Dr. H. M. Mixer, examined by Mr. Thrasher.—I am a "Doctor," and have had a legal right to call myself so about five years. I received my medical education partly at the Cleveland Medical College and partly at the Berkshire Medical College. I have practiced at Chardon, Geauga county, four years in partnership with Dr. Hamilton—the last year alone. I have not turned my attention to the subject of poisons specially until since I was engaged by Mr. Thrasher to make examinations and analyses in this case. Had previously devoted about such an amount of attention to the subject as medical men ordinarily do when engaged in practice. About the middle of February, 1858, Mr. Thrasher brought into my office a semi-putrid mass of animal matter, which he said was a portion of the remains of Mrs. Adelia Cole. I should not be able to say from their appearance to what part of the viscera they belonged, but was told by him they were portions of the liver and intestines. He said he wished an analysis made of them by Dr. Hamilton and myself. I was told we had less than half the liver—the other portion having been taken by the State to Prof. Cassel, at Cleveland. The liver and intestines were contained together in a glass jar, and with them a quantity of fluid. In this condition judged that nothing would be gained by separate analysis, and therefore tested the whole together for arsenic. We found it in perceptible quantity, but can form no estimate as to the amount obtained. I cannot say how long arsenic will remain in the body after being taken in less than a fatal dose. I do not think the question definitely settled. Some animals upon which arsenic was comparatively innocuous, and to which the poison had been given in large quantities, have been found with arsenic in the liver twenty or thirty days after taking the dose; but I do not believe this establishes a rule upon which reliance can be placed with reference to its effects upon the human animal. I gave to a dog one grain of arsenious acid. Ten days after I killed him. There was no arsenic in the liver, at least I found none. I used the Marsh process.

To another dog I gave 1 gr. of arsenious acid, and 6 days subsequently killed him. The Marsh process in my hands failed to detect a trace of arsenic in the liver. Having emptied the bladder, I swallowed 25 drops of Fowler's Solution. Six hours afterwards I

for the first time after taking the dose voided urine. It contained an appreciable quantity of arsenic by the Marsh test. I know these experiments are by far too few to furnish a basis for an opinion—still I incline to the notion that a medicinal dose of arsenic would be eliminated from the human system much sooner than the time commonly supposed to be required. Dr. Hamilton superintended in person the second disinterment, and brought back 3 jars of material from the body of Mrs. Cole. The first contained 1 pound of muscle taken from the inside of the thigh. The second held one lung and a small quantity of blood lodged in the cavity of the thorax. The third contained a portion of the spinal column, including bone, muscles attached and "spinal marrow," in all I should judge about 5 pounds. The muscle of the thigh was in a perfect state of preservation. I could not judge so well about the muscles of spine, as they were bruised some in process of removing them. I judged, however, they were not so well preserved. The analysis of muscle of thigh was commenced immediately after its reception, and was conducted by Dr. Hamilton and myself. The examination of spine was deferred about three weeks in consequence of the absence from town during part of that time of Dr. Hamilton. We obtained arsenic from the muscle of the thigh by the Marsh test. I think we obtained 5 or 6 mirrors upon porcelain from this muscle. In the examination of spinal column for strychnia, we pursued the Stas method. We did not find in it strychnia. We did find morphia. Finding morphia and failing to detect strychnia, would in my opinion render it highly probable that strychnia was not present. Still it may be true that one of the alkaloids is decomposed in the tissues of the human body while the other is not. So far as I know strychnia has never been detected in the tissues of the human body. I suspect when failures have happened they may be due to the fact that patients have been medicated with morphia, and this would destroy the color tests. This is only conjecture—the question is not yet settled.

I ought to say I had never before analyzed any portion of the human body for the vegetable alkaloids. I had previously examined portions of dogs known or supposed to have been poisoned with strychnia, and had never failed to detect it. We dug up a dog that had died under circumstances which led us strongly to suspect he had been poisoned with strychnia. He had been buried 4 months and putrefaction was very far advanced. We found strychnia in

both the stomach and liver. Before making an analysis of the body of Mrs Cole we had killed 5 dogs with strychnine, and in all of them had found the poison. We had tested the liver, blood, contents of stomach and spinal cord. We did not examine all these parts from the same animal, but ordinarily more than one of them. The largest dose given to any one of them was 1 gr. and generally much less was sufficient to destroy life. To one dog we gave 1-6 of a gr. of strychnia, and an hour afterwards we gave 1-5 of a gr. It was a large dog, weighing I should judge some 60 or 80 pounds. The poison was given in lard. He died in five minutes after taking the second dose. I thought the first dose probably killed the animal. We removed for the purpose of analysis the spinal cord and from it obtained strychnia. In one experiment we thought we obtained more of the alkaloid from the spinal cord than from a like amount of tissues taken from any other part of the body. I think the color tests reliable. We proved the crystalline residuum obtained from dogs to be strychnia with the same certainty that we could establish the identity of the strychnia of the shops. We tested them side by side. We had a much larger quantity of material from the body of Mrs. Cole upon which we operated, than we ever used in testing for the poison in animals. It was also the part in which we thought we found strychnia most abundant in animals. In the light of our experiments I think I can safely affirm that strychnia can be detected in the bodies of dogs when taken into the stomach in minimum fatal doses. This would not be conclusive evidence that it could be detected in the human body, but in my judgment would render it highly probable. I feel confident that the doctrine soon will be that strychnia in fatal doses must be detected in the tissues of the human body.

I know no reason why there should be a difference between the human stomach and the stomach of animals as to the decomposition of strychnia. In dogs strychnia is undoubtedly absorbed into the blood vessels directly from the stomach without undergoing digestion or decomposition. I think this will be found equally true in man. We had no lens with which to examine the residuum obtained from animals, and cannot say from the form of crystal that we obtained strychnia. I think the color tests reliable—I know of no source of fallacy in them. I mean they are conclusive when obtained. A failure to obtain them would not show absolutely that the substance under examination was not strychnia, because the presence of mor-

phia interferes with the reactions. We had difficulty in finding strychnia when in combination with morphia—a difficulty we did not entirely surmount. A solution of tannic acid in presence of a solution of a salt of strychnia gives a white precipitate, and this is not interfered with by the presence of morphia. Other substances may and do produce a white precipitate in the presence of solution of tannin, so that the obtaining of the precipitate does not establish undeniably the presence of strychnia, whereas a failure to obtain the precipitate does, so far as I know, prove the absence of the alkaloid. With this test we found—so far as the test goes—strychnia in animals poisoned with strychnia and morphia. This test we also used in our examination of the remains of Mrs Cole.

The symptoms of strychnia poisoning are not uniform. The most prominent symptom, and almost the only one invariably present in the animals upon which we experimented, was difficulty of breathing. In a few minutes after taking the poison, (I should say immediately after the absorption commenced) although it was cold weather—the dogs would be lolling, and this disturbance of the respiration would increase until during the height of the spasm—breathing would be entirely suspended. This difficulty of breathing ceased only when death took place. It was so apparent as to attract the notice of any one who should be present Normal breathing never returned after the poison first produced symptoms.

The animals were never thirsty while laboring under the effects of strychnia. We repeatedly offered them water—both in a dish and by spilling it on the floor; but they would go away from it—seemed to loathe it. Did not notice an instance of rigidity after death, in any of the dogs. Thought the ordinary "rigor mortis" was delayed by the poison, though I confess I do not know at what time it takes place naturally in dogs. In two or three instances, I did not remove the portions destined to be used in our analyses until 12 or 15 hours after death, and at that time, always found the bodies flaccid. I did not, in any of them, observe unusual heat, and from theory, should not be able to account for such heat as a result of strychnine poisoning. I have no theory at all satisfactory to myself, as to the manner of death from strychnine. I have seen Mr. Harley's theory, yet can not see clearly how all the phenomena can be accounted for in accordance with his views. My own opinion is, that the manner of death is not yet fully understood. The origin of animal heat is not well understood. Condensation and chemical

action are both accompanied with the evolution of free caloric; and from these processes taking place in the body, it is generally supposed heat is derived Both these processes take place in the lungs during respiration, and I should be unable to account for unusual heat in the body during a disease which excludes from the lungs a large portion of air usually furnished to them. Strychnine is intensely bitter to the taste—more so than any other known substance—leaves a persistent bitter taste on the back part of the tongue and faucies. Think no one could swallow it in solution without experiencing the sensation of intense bitterness. Think it could not be taken in a spoonful of liquid, and disguised; nor in a cup of coffee, nor in Epsom salts.

Involuntary micturition was present in all the animals to which we gave strychnia. In females, the stream of urine would be thrown off with considerable force, completely emptying the bladder.

In poisoning with strychnia, the intellect is usually clear. It is so in other forms of tetanus. Indeed, I believe unclouded intellect ordinarily attends all forms of tetanus, however induced. I do not understand that it is peculiar to strychnia. I can not say definitely what is a fatal dose of opium. Two grs. is a large medicinal dose, and when we go beyond that, we are approximating dangerous doses. I should not give or take six grs. of opium in the absence of disease. I have seen one case of traumatic tetanus. Sensitiveness to slight external impressions, is common to all forms of tetanus, and is due, of course, to the super-excited condition of the nervous system. After tetanus is induced, I do not think I have any landmarks by which I should be able to distinguish between idiopathic, traumatic, and poison-tetanus. I mean during the spasms. I think, taking the whole history of the case, an intelligent physician would ordinarily arrive at a tolerably correct conclusion. Opium sometimes produces tetanic convulsions. A case of morphia poisoning came under my observation during the last summer. A young lady took, through an error in the judgment of her nurse, about two grs. of morphia. No narcotic symptoms were developed, and indeed no symptoms of any kind, for about 14 hours, when she was seized with well marked tetanic spasms. There were two or three spasms, and during their continuance, the patient rested on her head and heels.

To ascertain the solubility of arsenious acid in laudanum, I set

aside in my office, in one vial, $\frac{1}{8}$ of a gr. of the acid in one drachm of laudanum; and in another drachm, $\frac{1}{16}$ of a gr. These I agitated daily for one month. The acid did not dissolve fully in either vial. I analyzed ten portions of the horse medicine—one drachm each time. I found that one drachm of the horse medicine contained $\frac{3}{8}$ of a gr. of arsenious acid—doubtless a part of it in suspension. I measured the spoon from which Mrs. Cole took her medicine. It held $1\frac{1}{2}$ drachms. This spoonful of the horse medicine would, therefore, contain $\frac{9}{16}$ of a gr. of arsenic. The smallest fatal dose of arsenic of which I have any knowledge, is a little less than two grs. Should not think $\frac{1}{2}$ a gr. of arsenic and $1\frac{1}{2}$ drachms of laudanum, a safe dose. Certainly should not like to take it.

Dr. Hamilton brought into my office a vial of laudanum and arsenious acid, mixed in such proportions as to resemble as nearly as possible the horse medicine. I agitated this thoroughly, poured off a teaspoonful, (the teaspoon from which Mrs. Cole took her dose,) allowed it to subside, poured off the laudanum and dried and weighed the residum, and found I had thirteen grains. Less than two grains is a fatal dose. Arsenic usually begins to operate in about half an hour after being taken, though circumstances may greatly vary this. It has taken effect within ten minutes, and its operation has been delayed ten hours. Death is ordinarily produced in from eighteen to thirty hours.

Tetanus produced by a wound is called traumatic, and is supposed to result from irritation of the nerves of the wounded part. I see no reason why arsenic, by irritating the nerves of the alimentary canal, may not produce tetanus as its first symptom, and the books teach that such is the fact. The symptoms of arsenical poisoning are not uniform. Indeed, there is almost no symptom to which diseased flesh is heir, that may not sometimes be produced by arsenic. I do not know what symptoms would be produced by arsenic and laudanum mixed in certain or uncertain proportions. I have no data upon which to form an intelligent opinion as to their combined action. When death is caused by arsenic the stomach and bowels are usually found inflamed, but not always. Nor is arsenic always found in the stomach. Fatal dose of arsenic ordinarily causes a diminution of the secretion of urine—sometimes an entire suppression. When a fatal dose has been taken the symptoms are no guide as to the size of dose. In arsenical poisoning there may be a remission, and even an intermission, varying in duration from a few minutes to a couple of days; and in a fatal case, if symptoms returned, I should

expect it would be with increased violence. I am not able to account for remissions or intermissions. So far as I know the fact is ultimate. All the authorities teach the doctrine and give cases, but none of them explain it. With our present knowledge it probably does not admit of explanation.

The first treatment should be to rid the system of the poison. I do not think coffee would have much effect upon a person laboring under the influence of a fatal dose of arsenic. It is no antidote, and would not impede the work of death except by sustaining a little longer the failing energies of the system. Epsom salts contain, as an impurity, ordinarily chloride of lime, soda or magnesia. By the action of the acid of the gastric juice upon this impurity, chlorine would be set free in the stomach, and from its known extensive range of affinities for organic compounds, it is possible oxygen would be liberated, which, in its nascent state, uniting with arsenious would convert it into arsenic acid, a more virulent poison. It is mere theory, upon which I do not place much reliance. Still if I were prescribing a cathartic under such circumstances, I should not order epsom salts with such impunity.

I examined the package of salts from which Mrs. Cole is said to have taken her dose, and found that it contained some form of chloride, as also did all the specimens of salts to which I had access. In analyzing the remains of Mrs. Cole we designed to proceed with the utmost care, so as to avoid every possible source of fallacy.

What do you say as to the possibility or probability of a female of feeble health, nervous constitution, and with a system peculiarly susceptible to the effects of poisons, having taken a drachm and a half of laudanum, with from two to six grains of arsenic, experiencing the following symptoms, to wit: Tetanic convulsions in three-fourths of an hour after poison taken, which continue more or less severe for from four to six hours, with remission of, more or less quiet, of from ten to twelve hours, with a return of convulsions and other distressing symptoms, and death in twenty-six hours? Might or might it not occur? From the known effects of the poisons, when taken separately in fatal doses, I see nothing improbable in the production of such a train of symptoms.

Cross-examined by Mr. Riddle—I never witnessed a violent poison produced from a dose of salts. In the examination of the muscle of the thigh, we continued in operation the Marsh apparatus as long as we obtained the arsenical mirrors. We tested the mirrors to determine that they were arsenic and not antimony. I could not

from the test made form a very accurate opinion as to the amount of arsenic in the body. The test determines the presence of very minute quantities. I do not pretend to say that we produced crystals of strychnia which I could declare strychnia from their form—I only said, or designed to say, that we obtained from animals a crystaline residuum, which, by the operation of the color tests we identified as strychnia. I do not say the color tests are infallible—we can say that with reference to very little of our knowledge, I am not aware that there are in all organized matter substances which produce with the same re-agents, the same color as strychnia. I should rather have material to make more than one color test; still if the changes of color peculiar to strychnia were produced perfectly and distinctly once, I do not know why that should not be regarded as conclusive. I made experiments to ascertain if the taste of strychnia would be disguised by mingling it in solution with Epsom salts, and found that it could not be thus disguised. I never said that the taste of strychnia is disguised by salts—I have heard such statements from others, and made my experiments to satisfy myself of their accuracy. I cannot say what symptoms would result from the exhibition to a delicate female of a drachm and a-half of laudanum with from one to three grains of arsenic. I have no premises on which to base conclusions, and should rather be excused from theorizing upon the subject. They do not chemically combine to form a new compound. One laboring under the irritating effects of arsenic would bear larger doses of opium or any of its preparations, than when free from disease.

I know that the exhibition of Dovers powders produces effects quite different from the effects of the materials of which it is composed, when given separately, and though I have an idea as to what symptoms would usually be produced by arsenic and opium, given separately, I cannot, by analogy or in any other way, determine what would be their combined effects.

The animals we poisoned with strychnine, were not thirsty. They manifested an inclination to avoid water before there was rigidity of the muscles, which would prevent their drinking. I do not know—and never said I did—that strychnia is not decomposed in the tissues of the human body. Think Taylor, in his review of the Palmer case, advocates that theory.

Question. After the second disinterment of Mrs. Cole, (which was conducted by Dr. Hamilton,) and when the remains had been brought to Chardon, did not Mr. Forrest, as one of the attorneys for

the State, propose to you that the analysis should be conducted by Dr. Hamilton and yourself, but that the result should be verified and witnessed by a chemist procured by the State? He did not. Dr. Hamilton brought away a large quantity of material, with the expectation that it was to be divided with the State. During the negotiations between Mr. Forrest and Mr. Thrasher, pending this division, I visited Mr. Forrest's office. He did propose that the analysis of the material should be commenced by us, and when the State had decided whether it wished for part of the remains, he asked a promise on my part that the material, in whatever condition it chanced at the time to be, should then be divided with them, if they desired it. I told him that no chemist would surrender material in such condition, and none would consent to receive it; that results so obtained, would be second hand and unsatisfactory.

Do you state upon your oath, that in Mr. Forrest's office, and in the presence of Mr. Canfield, he (Mr. Forrest,) did not propose to you that the State be permitted to procure a chemist to witness the result of your analysis? I do so state most unequivocally, and I say further, that, had such a proposition been made to me, it would have been declined. Our investigations were made by procurement of Mr. Thrasher, and the results were his property. I should have had no right to divulge them to the State's chemist, or to anybody else.

Was not the fact that your investigations were to be kept secret, made by Mr. Forrest the ground of objection upon which he declined altogether to have anything to do with the remains? It was not. (Mr. Thrasher here read the letter written at that time to himself, by Mr. Forrest, in which he (Forrest) bases his declension upon other and entirely different grounds, and never so much as refers to the fact that the results of Hamilton and Mixer's investigations were not to be disclosed until the trial of the case.)

Did not all the animals, poisoned by you with strychnia, have spasms? They did. Was there not present with all of them, extreme sensitiveness? There was. Did they not all have lock-jaw? They did. Well, then, when you say that difficulty of breathing was the only constant symptom, how are we to understand you? I did not say that difficulty of breathing was the only constant symptom. I said it was the most prominent symptom, and said also, in substance, that if any symptom was PECULIAR to, diagnostic of strychnia, I thought difficulty of breathing that symptom.

Re-examined by Mr. Thrasher. I confess I was surprised to learn

that a man so eminent in the profession as Dr. Taylor, had verified the truth of the doctrines advanced in his writings upon the subject of strychnia, by experiments upon such a limited number of animals. Dr. Christison regards taste as the most reliable test for the presence of strychnia. I do not understand that the reactions of strychnia and morphia are the same in any case. Nitric acid reddens morphia, and also brucia; and the ordinary strychnia of the shops, contains brucia. But nitric acid does not redden strychnia, and no one uses it as a test for that poison.

NINTH DAY.—THURSDAY, NOVEMBER 25.

Dr. Mixer recalled.—Examined by Mr. Thrasher. Upon reflection, and consultation with my associate, Dr. Hamilton, I am convinced I was in error in stating, that from the muscle of the thigh of Mrs. Co'e we obtained only five or six mirrors. I now recollect that we used but half the muscle at a time, making ten analysis, and from each portion we obtained five or six of the arsenical mirrors. There was a livid appearance of the tongue and lips in the dogs, to which we gave strychnia. I did not observe with reference to this in the first two or three, but in the later ones this was very apparent. It was manifest very early, as soon as difficulty of breathing came on. With some of the dogs there was spasmodic closing of the jaws, alternating with relaxation, producing an almost constantly recurring snapping noise. During the general spasms, the jaws were locked and still.

Cross examined by Mr. Riddle. In testing for strychnia, I did not make use of the physiological test. I have confidence in the test. In it we rely upon symptoms.

The case was elaborately argued by counsel, the argument occupying two and a half days. Having been twelve days in progress, the case, under charge of the Court, went to the jury at 3 o'clock, on Saturday, November 27. On Sunday morning, having been out thirteen hours, the jury returned a verdict of "not guilty," and the prisoner was discharged from custody.

A Case of Gun Shot Wound. With remarks by H. CULBERTSON, M.D.

On the 6th of February inst., six hours after the accident, I was called to see Norvill Speed, a healthy young man aet. 18, of nervo-lymphatic temperament, who had, while sporting, accidently shot himself in the abdomen. The wound presented all the characteristics of a near-wound inflicted by small shot—was five inches in length, and two in breadth, the orifice of entry the size of a quarter, three inches below the umbilicus, and two and a-half to the right of the median line. The wound laid between the superficial fascia and the adipose structure beneath the skin, its superior extremity reaching the median line, so that its direction was up and across from right to left. When the accident occurred patient had on two shirts, a pair of drawers and trowsers, all of which were perforated by the shot. On probing the wound carefully could feel no wadding or clothing. He presented the following symptoms: mind rational, is hopeful, general surface pale and cool, extremities cold, no pain, feels weak, pulse seventy-six, small, soft and weak—has twice vomited bright red blood, to a large amount, in the clots of which were found three rough and flattened shot. No discharges from external wound save a little blood. Has micturated with pain, but urine contains no blood.

Diagnosis. Gun shot wound between the integument and superficial fascia, and of the middle of the stomach.

Nature.—The dense superficial fascia favored by the nearly vertical position of the gun when discharged, has been the means of causing the load to glance up beneath the skin, until by the resistance of the adipose structure, the wadding and cloth, with the shot behind, were impeded in progress, and some of the flattened shot passed into the stomach, producing, probably, an incised wound of that organ. It is doubtful whether the bowels are wounded.

Prognosis.—Unfavorable, but still encouraged the family, saying "it might turn out well." Indication of cure—To prevent further bleeding, and bring on a moderate reaction.

Treatment—A tight abdominal bandage and compress beneath, with an opening at seat of external wound—an oil-cloth over the entire abdomen, and on this cold water dressing—external wound

dressed with lint—grain doses of pulv. opii every three hours, and if pain requires, more frequently. If sleeps, not to be troubled—no food. Water in tea-spoonful doses every fifteen minutes—no visitors—dark and cool room—perfect rest in recumbent position.

Feb. 7th.—Since my last visit, has slept several hours—has no pain or fever—very little thirst—no discharge from wound—moderate reaction—pulse 100, irregular, and more full, but still soft.

Indication to keep down reaction by cutting off the nervous sympathies, thus preventing the presence of an undue amount of inflamation, also to keep the bowels in check, by larger doses of opium.

Treatment of yesterday continued.

Feb. 8th.—Has slept several hours since yesterday. Has no pain—very little fever—no thirst—micturated without pain—urine scanty and high colored—some pus on edges of external wound.

Indication of cure the same. Treatment—Applied a new abdominal bandage—enjoined rest, absence from food, and allowed but little water. To continue cold water dressing, and opium as above, unless asleep.

In the evening heard the patient had vomited once, throwing up bile, and also that he complained of some soreness around external wound. The other symptoms as formerly. Treatment continued.

February 9th. Symptoms still about the same ; complains of some motion and soreness of bowels, no vomiting to-day, has expectorated some mucus, *but not colored with blood*, sleeps well, pulse 84, somewhat hard.

Treatment. Continue opium as occasion may require. Ant. Tart. in solution gr. one-tenth every three hours, with direction to cease giving it if it produced too much nasuea or vomiting—other measures as before—still no food.

February 10th.--Symptoms still improving; no pain, fever or thirst; slough commencing to separate at external wound; pulse 76 and softer ; has not vomited ; does not require so many powders ; sleeps well ; tongue coated with white fur ; is hungry. Treatment—continue powders occasionally and Ant. Tart. every three hours in same doses ; apply bread and milk poultice to external wound : continue bandaging and cold water dressings. Gave as food and drink muc. gum acacia in table spoonful doses every half hour.

Remarks.—I notice that the inflammatory process has developed the presence of shot beneath the skin, but do not yet feel any wadding or cloth.

On the 11th his pulse was 72, natural. Gave nothing but a pow-

der of opium, discontinued antimony and continued the other means. The urine is more copious, and loaded with urate ammonia. Gave for food, milk diluted one-half with muc. gum acacia.

12th.—Symptoms much the same, except that he complains of some soreness of bowels and external wound; pulse 68 and natural. A dark serous discharge, colored by the gunpowder is coming away from the wound. Treatment continued.

13th.—Symptoms much as usual; pulse 76, natural. At the upper extremity of the wound can feel a hard body. This I suppose is the wadding (brown paper) or a portion of his clothing. On pressing on track of wound there came out about half ounce of the above mentioned fluid, which contained some air bubbles. Took yesterday but two powders. Treatment—To take powders as needed; discontinue cold water dressings, continue poultice, and take sago prepared with water for food.

Remarks.—The indication to-day is to remove the foreign bodies from the wound; but my forceps not being long enough, and after failing with a bent knitting-needle, I was obliged to defer this operation until I could make an instrument. I did not deem it proper to make a direct opening.

Feb. 19th.—From the 13th to this date the symptoms have gradually improved. Pulse ranging from 60 to 72.

On 17th had a free evacuation from bowels, slightly reddened by blood, brought on by an injection of salt and water. Thus he passed 11 days without defecating.

On the 14th endeavored to remove the wadding, &c., from wound, but did not succeed.

On 15th made a canula of German silver, in which slid a three-pronged steel wire, the points of which were sharpened and bent at a right-angle, and so arranged that by pushing out the handle the ends would spring apart and embrace the object, and on drawing back the handle, would close and hold it securely as the instrument was withdrawn from the wound. With this I succeeded in removing 8 shot, and all the cloth and wadding, except a small portion which seemed to be firmly embedded in the substance of the skin.

The microscope showed the material removed from wound to be cotton. The instrument referred to, has the advantage of not giving pain in manipulating, as is the case with forceps when the blades are opened and handles spread apart to secure the object. A hook is troublesome by catching in the sound tissues, besides not being efficient, and gives more pain than a large sized canula.

ding or cloth.

On the 11th his pulse was 72, natural. Gave nothing but a pow-

On the 15th, allowed some chicken once, and finding, on 16th, that it did not injure him, ordered it three times daily, as he needs food after his starvation.

On the same day, allowed him to set up fifteen minutes, which he has continued to do every day, gradually increasing the time.

The discharge now (19th,) is healthy pus, and granulations are springing up in the mouth of the wound.

From the 19th until to-day, (the 22d,) the case has progressed favorably, and will continue, I hope, to do so in future.

The case presents several interesting points:

1st. No one could determine in its early stages the exact amount of injury present. The bowels were not injured, neither the lungs or liver, as is shown by the sequel; yet there was good reason for supposing these organs might be.

2d. It affords another illustration of the fact that the membranous structures often cause the missile to take a new line of direction.

3d. When we consider the near proximity of the gun to the body, (only a few inches) it seem strange that no more injury was inflicted.

4th. I doubt whether there have been many cases of abdominal gun-shot wounds, of like character, so free from unfavorable symptoms.

5th. I would only mention the prevailing belief, that success in the cure of such cases, depends upon (so far as the physician's efforts are concerned,) the administration of large doses of opium, even although there is no pain, (for the remedy blunts nervous sensation,) aided by the lancet, antimony, abstinence from food, and the too fashionable exhibition of laxatives, and even purgatives.

6th. The wound of the stomach has probably healed by first intention, and there may or may not be peritoneal inter-union.

P. S. On visiting this morning, (23J,) I find that on the evening of the 20th, after much nausea, he vomited up about half a pint of dark blood, very firmly clotted, and passed black blood by the bowels, after which, he felt much relieved. This, evidently, was old blood, and no doubt its presence in the stomach not only prevented the flow of blood and fluids into the peritoneal sack, but acted as a shield beneath which the wounds of the organ healed. It (the clot) probably was held in situ by partial contraction of the stomach. His symptoms are still favorable.

Zanesville, Feb. 24th, 1859.

is troublesome by catching in the sound tissues, besides not being efficient, and gives more pain than a large sized canula.

PART SECOND.

AMERICAN AND FOREIGN INTELLIGENCE.

BINOCULAR VISION.—Of the thousands who gaze with delight upon the magical effects produced by that small instrument known as the stereoscope, how few there are who comprehend, or attempt to assign reasons for, the extraordinary optical illusions experienced through its instrumentality.

It is with the view of, in some degree, elucidating the principles of vision upon which these are founded, that the following article is written.

It will in the first place be well to consider the difference between monocular and binocular vision. Nature has furnished us with several means of determining the distance of objects which may happen to come within reach of our visual organs. One is that of distinctness; a greater or less degree of which—other things being equal—gives an idea of greater or lesser distance in the object viewed. The second is through the change of focus required in the lens of the eye in refracting to a point on the retina, rays of light entering it with a greater or lesser degree of parallelism, thus producing in the brain a consciousness of unequal distances in the objects from which they emanate.

The means above alluded to, it is evident, are enjoyed in almost the same degree, when viewing with one eye as where both are used.

By far, however, the greatest power with which nature has endowed us of discriminating distances, is through the agency of binocular vision; or in other words, in the sensation produced in the brain by the different degrees of convergency of the optic axes required in obtaining distinct vision of the differently distant points of objects upon which they are directed. It is to this faculty that we are indebted for our most palpable evidence of differential distances and for that consciousness of solidity and relief so remarkably experienced in the stereoscope.

It is evident, for example, when we are looking at a house or other object that has depth as well as breadth, from such a point of view as to enable us to see two sides of it at once, that we receive a differently perspective image upon the retina of either eye, or that we must see more of one side and less of the other with the right eye than the left, or vice versa. Thus accomplishing with one view what a person with but one eye would require two views at positions two and a half inches apart—the distance between the eyes—to accomplish. These are the different perspective views of the stereoscopic cards, and it is the effort to reconcile these dissimilar pictures by converging the optic axes at points differently distant from the eyes which produces the wonderful effects above alluded to, and which enables us to experience all the sensations of delight

which would be produced by the contemplation of the landscape itself.

The stereoscopic pictures will of course never quite correspond. They are taken simultaneously with a camera constructed with two lenses, or consecutively with a camera with one movable lens.

The lenses of the stereoscope, besides magnifying the pictures, are so placed as to unite certain similar points of them, thus relieving the eyes of too great effort by uniting them entirely by convergency of the axes.

The means above alluded to, by which we are enabled to judge of differential distances, are of course much diminished by the distance that the objects viewed are removed from us.

Our consciousness of different distances by distinctness is diminished through decrease of light.

Our judgment, through change of focus, is diminished in consequence of the parallelism of rays from distant objects being so nearly the same as to require but little change in refracting them to a point on the retina. And lastly, the binocular effect is in a great degree impaired through the identity of distant views when seen from positions only separated by a base of two and a half inches.

Nature has thus observed her usual economy in providing for our necessities alone. It being of little comparative importance to us generally, to be acquainted with the relative positions of distant objects, whereas our personal convenience and even safety depend greatly on our knowledge of those near at hand. We are therefore provided with much more ample means of determining the latter than the former.

It may not be out of place here to allude to another subject immediately connected with vision.

Sir David Brewster says, in allusion to the cause of erect vision from an inverted image, "That it has long been a problem among the learned." And further remarks, "That it is perfectly explained by the law of visible direction." Now, although it is evidently a consequence of that law, the question arrises, why should we see objects in a direction perpendicular to the retina at the point where their image meets it?

In truth, it is one of those facts that requires no explanation, as there is nothing inconsistent or irrational in it.

There is no more reason that an inverted image should give us the idea of an inverted object, than that the reverse should be the case, or that it should give the idea of a horizontal one. There is no unity necessary between the direction of an object and the direction of its image upon the retina. Nature has so associated impressions upon the retina with impressions on the brain, that an inverted image on the former is evidence of an erect object to the latter. And we have no consciousness of anything to the contrary. —*Jour. Frank. Ins. Dec.* 1858.—*Jour. of Pharmacy.* W.

Francis' Introductory.

The annual course of the winter session of Clinical instruction at this great Charity of New York, was opened on the 18th of October, 1858, when the introductory lecture to the several courses of clinical illustration and practice was delivered by John W. Francis, M. D., LL. D., the President of the Medical Board, in the new and spacious operating theatre recently finished. The discourse was delivered to a crowded assembly of medical men and students of medicine, many from the remotest parts of the Union, and was received with deserved approbation. It embraced among much matter of a collateral character, a history of the origin and progress of anatomical preparations, and illustrations of the estimation in which the art was held, and its continued importance from the time of Swammerdam and Ruysch, of Holland, down to the latest period of science. We extract from a portion of the discourse a brief selection of occurrences, including the times of the Hunters, down to the transactions of a like nature in New York, and in some of the other institutions of the republic. We have learned that this address, at the request of the students, is about to appear entire from the press:

Pregnant with glory as are the annals of British medicine and surgery, and enduring as must be our gratitude for the advancement our profession is admitted to have received from English science, there are two names that stand above all others of that empire to whom must be awarded a special pre-eminence for their labors. I allude to William and John Hunter. As anatomical and physiological pursuits are now conducted, we are compelled to revert to these great men as the most successful explorers of the human structure during the march of modern improvement. Their lives have been amply written, and at a future time you may enrich your memories by a perusal of their achievements. Notwithstanding the advances recently made in the same subjects of inquiry which once summoned their close attention for years, their writings are still perused as every-day books. Rigid dissection and minute anatomy are the grounds upon which their merits rest; and seldom, indeed, in this our own time, does much controversy arise out of their revelations. While living they met opposition enough, but the arguments, *pro* and *con*, have settled in the conviction that an almost Baconian accuracy characterizes their deductions. This is especially the case with the writings of William, the elder brother. His 'Anatomy of the Gravid Uterus' is the rock upon which his fame rests. No subsequent writer has attempted to rival it, and though contentions have sprung up as to his account of the membrana decidua et reflexa, I am satisfied by occular proofs of his preparations, that he had the best reasons for his description.

His minute and elaborate work was the result of twenty-five years' labor. I cannot speak of his other writings on this occasion. He was every inch of him a philosopher. Of John Hunter, his writings announce the originality of his mind: he was a practical sur-

geon; as a deep physiologist and pathologist he did more than even his renowned brother, and his name is consecrated in the history of medical transactions. But I must not usurp the duties of your teachers. I must leave to Dr. Mott, and to Dr. Van Buren, to Dr. Dalton, and Dr. Metcalfe, and Dr. Clark, and your other instructors, the task of specifying his merits. With little learning, but an untrammelled intellect, John Hunter discovered, with wonderful acuteness, unsuspected truths, and his reflections were marked by a rationality which generally led to the strongest convictions. What raptures must have filled his soul when he saw that his discoveries had secured a deathless fame. How he must have revelled in the luxury of genius. He was poor, but were not his possessions greater than those of emperor or king, and could he not, in defiance of worldly pomp, exclaim: "My mind to me a kingdom is?" He had spent more than ninety thousand guineas in anatomical investigations. Garthshore, one day entering his museum, and finding him busily engaged, exclaimed: "Oh, John, you are always at work." "I am," replied Hunter; "and when I am dead you will not soon meet with another John Hunter." His copious volumes are full of practical import. He was a mighty reformer in surgical doctrines, and in physiological and pathological principles. He was of no school. He was nature's interpreter. I can hardly say in how many branches of our art he was an innovator and discoverer, as well in comparative anatomy as in vegetable physiology. He profoundly investigated the nature of life and the origin of disease. He passed almost an entire ten years, at one period, in the dissecting room, sometimes aided by Cheselden, sometimes by Percival Pott. Abernethy delighted to recount stories of his peculiarities, to which I have repeatedly listened; of his provoked mind, from a want of command of language, correctly to set down his thoughts; and of his determination to allow himself but four hours sleep at night, while after his dinner he generally took a siesta of some two hours, though the books, said Abernethy, affirm only one; and this plan was adopted in conformity to his physiological laws, that digestion was a great function, and demanded repose.

He who would know how liberally the Hunters appropriated their hard earnings of professional life, and the incredible amount of good they accomplished for the advancement of medical knowledge, will profit by their biographies, and reverence both the heads and the hearts of these illustrious men. Like the Bernoulli in mathematics, they have earned an immortality of fame. Their claims to the regard of posterity are unbounded. From humble life and extreme obscurity, they were by individual effort raised to the highest eminence in the scientific world.

Their reputations, however, do not depend upon biographical facts, nor even on their precious writings; these, indeed are accessible to all, and all may grow wiser by them. They have left the monuments of their renown, erected by their inflexible perseverance, in the great collections which they formed of anatomical preparations, and as these are open to the inspection of the world, the

Hunterian Museum in Glasgow and that in London will speak in louder accents in behalf of their founders than the eulogy of the warmest friend, or the most erudite mind.

Sir Everard Home has published quartos descriptive of the Hunterian collections. As these museums are objects of inquiry to all travelers, I, of course, could not forego the pleasure of a sight of them; they, moreover, fell within the scope of my professional business. To examine them with profitable results, would have demanded months, nay years. Even a partial survey strikes the beholder with wonder at their magnitude; and their exquisite beauty and perfection almost discredit the idea that they are the work of mortal hands. Wherever there was life, Hunter sought out its organization, the better to unfold its phenomena. He stands alone. Were I to estimate him by his genius and his studies, I would not hesitate to class him among astute theologians, so sublime were his pursuits, to vindicate the ways of God to man. "To prove an intelligent Creator," says Archdeacon Paley, "I take my stand in human anatomy."

From the monad, or primary molecule, to the perfect formation of the mature being, all is illustrated; the most minute specimens of sound and morbid anatomy are brought to view, and every article arranged in its relation with the laws of organic life and development. Here, I repeat, the man of research will find every specie of animated being, or link in organization, from the humblest vegetable to organic man. What a revelation of created wisdom is thus unfolded! All—all the work of two individuals. What devotion must have inspired John Hunter—what seclusion from the world and its gaieties must have reigned within his mighty bosom, so to absent him from society, in order to seize time for his vast work. I again repeat, the soul is penetrated at the sublime manifestations which weigh upon the heart by an inspection of these collections.

There are those who have compared the labors of John Hunter to the Principia of Newton, and of the number who have uttered this language is the late Joseph Adams; but this is going too far. There are others, composed of the baser materials of our natures, who with malign intent, have denied his services, and dwelt upon his unlettered cultivation; precisely after the manner that James Watt was pronounced to have done nothing for the perfection of the steam-engine, or Fulton to possess no merits in steam navigation. I will not insult you by recording the names of such miserable detractors; the poverty of my diction permits me not to characterize them better. Let Stygian darkness ever cover them even if they escape the vengeance of heaven. It was, doubtless, too much for Hunter's miserable opponents to endure, when the world rang with John Hunter's praises; as they could not secure to themselves the penalty of celebrity, nothing was left them but to imitate the owl who, we are told, declares the eagle blind. Hunter, though of an irritable temperament, with many struggles, endured all, conscious of his deserts. Tell me if any of you have, at any time, read either in fable or in his-

in the great collections which they formed of anatomical preparations, and as these are open to the inspection of the world, the

tory, that the elephant ever complained of the too great size of other animals.

The history of all art, the history of every science, yields this instructive truth, that great discoveries are, after all, the issue of preceding investigation; it is step by step that our art, in many hands, and by the workings of many minds, has arrived at its present exalted state. Read Bracker or read Whewell, you will find this fact abundantly verified in the history of philosophy: a partial exception may perhaps be found with Sir Isaac Newton on optics; and in the case of John Hunter it would seem that by an intuitive sagacity he at once seized upon essential principles not dwelt upon before, and disclosed to our comprehension new laws of organic life, and new curative means of relief. When Dr. Pearson led me to the apartment of St. George's Hospital, and pointed out the place where Hunter fell dead, with a disease of the heart, I felt as though I stood on a consecrated spot. Don't you think, after all, that his death was noble; he died vindicating the rights of a student. He possessed an impetuous nature, but he had the tenderness of genius.

The world has produced but one Aristotle, one Bacon, one Newton, one Franklin, one Washington, one John Hunter, men who have individually demonstrated what special acquisitions in assigned services may accomplish. Let us at all times, with charity, tolerate diversity of opinion, but let us unite in one sentiment, that John Hunter stamped the impress of original powers on the noble art of healing, and enlarged the sphere of its benefits. We may further affirm, that the acute mental perceptions of John Hunter, by the appropriation of his vast gifts, have given us, in his museum, a book of wisdom, which, like the inspired volume, may be deemed an emanation of profoundest thought, and which has taught man how fearful and wonderfully he is made; that only a Divine Architect could have been his creator; and while it admonishes us that the wisest provision is made for his proper continuance, tells us his economy is neither to be molested by the presumptuous, nor tampered with by the ignorant unacquainted with its mysterious harmony.

These few reflections on these great museums bring forcibly to my recollection the gratification I experienced, through the courtesy of the anatomical professor, from an examination of the collection of preparations of sound and morbid parts, in the College at Dublin, in 1816, brought together chiefly through the zeal and talents of Macartney. Next, I think, to those contained in the museum of the Hunters, they exhibited the nearest displays towards perfection in artistic manipulation and soundness of preservation. Macartney was long an efficient aid in St. Bartholomew's Hospital, with Abernethy, and the translator of Cuvier's Anatomy of the Brain.

Whether this address, prolonged beyond the limits of my first intention, has inspired you with the importance of anatomy, and in the right means of acquiring a knowledge of it, by dissection, by the use of the knife, the forceps, and your own faculties of observation, is the secret of your own bosoms. I might go much further to point out the value of dexterous manipulation in the dissecting room, pre-

with many struggles, endured all, conscious of his deserts. Tell me if any of you have, at any time, read either in fable or in his-

paratory to the skillful display of chirurgical tact in the sick chamber, were I to adduce other illustrations of the exalted condition of surgical anatomy and pathology, as it is recognized at the present day, and cultivated in Europe, and in our own country. But the question is no longer debatable. You have, very lately, a triumphant specimen of the results of labor and talent in that field in the works of Dr. Gross, now a distinguished professor in Philadelphia.

Leaving then the subject, treated indeed but very imperfectly, I I shall close with a few scattered observations on the history of anatomical preparations in the United States; so far only, however, as to impress you with the fact that something has been accomplished among us, and that the ardor which now presses forward the pursuit must effect results prolific of lasting benefit.

In a recent lecture I gave you some particulars derived from our early Dutch records, of a post-mortem examination of the body of Sloughter, the English governor of New York. This occurred as early as in 1691. It is the first autopsic examination that ever occurred in the American colonies. The governor, it was affirmed, had died by poison, but no proofs of that nature could be derived from the doctors' post-mortem testimony. The fact is, the governor died from a debauch; and it doubtless furnished the Dutch faculty with a gratifying spectacle to exercise their functions over a worthless ruler, whose moral and political life was at variance with the ethics of the good old burgomasters, and whose inebriation proved to be the death of the patriotic Leisler.

The earliest anatomical dissection for imparting medical knowledge was performed also in this city, on the body of a convicted felon, Hermanus Carrol, by Doctors John Bard and Peter Middleton, in 1750. Only two years after there arrived from Scotland, William Hunter, at Rhode Island. He was a near relative of Wm. and John Hunter. The emigrant Hunter was now only twenty-three years old; but he had studied with the elder Monro, and at Leyden. He gave lectures at Newport in the year 1754, '5, and '6. They were reported to have been of a high order of instruction. He was long a prominent practitioner and a bold surgeon; he died in 1777. Hunter's lectures were, doubtless, the first regular course of practical anatomy founded on dissections taught in America. We have no means of knowing what might have been the contents of his museum, as his library and other effects were dispersed by the revolutionary struggle.

There is a name in the English history of anatomical preparations most conspicuous and familiar to all who are acquainted with the biographies of Monro secundus, William Hunter, Morgagni, Mead, and others. I allude to Dr. Frank Nichol. He was born in London; had visited the schools of France and Italy; was graduated at Oxford, and finally became one of the physicians of the king. It was during the middle of the eighteenth century that he signalized himself by making fine and highly finished preparations, which attracted the attention of the faculty. I mention him specially, because a portion (I can not tell to what extent) of his great collection of

injections was given by his son, a member of the British parliament, to Ward Nicholas Boylston, of Boston, for Harvard University; but the year is not specified. As Nichol died in 1778, it must have been nearly at the close of the century. I have often heard Samuel Bard and Wright Post talk of Nichol's talent in that vocation.

A collection of preparations illustrative of some points in sound and morbid anatomy, found a location in Pennsylvania, for the Philadelphia school, about the same time, the gift of the late Dr. A. Chovet. This incipient movement to establish a museum of anatomical preparations in the great medical school of Philadelphia, we are told, has been followed by many distinguished laborers in the honorable work within late years; and, if report deceive us not, the enterprise of Dr. Goddard, and the capacity and zeal of that remarkable man, Dr. Leidy, have already given exhibitions of skill in that peculiar line of scientific and artistic power, which held out to the profession lasting results of highest value to anatomical knowledge.

An effort of no ordinary character to create an anatomical museum for the college of New York, was made by the late Dr. Richard Bayley, of this city. What attempts of that nature had been made by his predecessor in the medical school established in 1768, in which Dr. Clossy was appointed professor, we cannot affirm with accuracy. A portion, however, of what had been collected, as well as a portion of the labors of Bayley, were destroyed by the Doctor's Riot, which took place in this city in 1788. The imprudent exposure of a limb by the students, excited the populace; the mob broke into the building, the New York Hospital, where they found Bayley's valuable cabinet, which they forthwith heaped into carts, carried out, and triumphantly burned. Little was left; but to that little, the able Dr. Wright Post added specimens of his own skill; the collection was afterwards enlarged by additions imported by Dr. Post, and with awaked zeal, Valentine Mott, Henry W. Onderdonk, John Van Buren, and Guy Carlton Bayley, and others, increased the number. This valuable collection is now in the possession of the College of Physicians and Surgeons of this city, and I must refer you to Professor Willard Parker, for more minute details concerning a museum, the result of many minds and many hands.

At the time I first began my medical pupilage, there arrived among us, a Dr. Alexander Ramsey, who bore the repute of a distinguished lecturer on anatomy. He had studied with Cruikshank, of London, and was famed for his anatomical preparations. He was chosen to deliver a short course of lectures on anatomy and physiology, in Columbia College. Of his professional knowledge there could be no doubt; but his grotesque person, his unbounded vanity and hauteur, his arrogance and pomp, excited the risibles of the whole class. Crooked back, with neck scarcely half an inch in elevation, with a head of huge dimensions, arms lengthened as if of the simia tribe, and person not five feet in height, he seemed created by nature in one of her whimsical moods. He was ever illustrating the harmonies of creation by reference to his own symmetrical development, and to the philosophy taught him by his master, Cruikshank.

His career was short among us, but some of the old doctors, still flourishing, must retain a recollection of him. He was called at times the Calaban of science. The ridicule which he provoked while teaching, forbade the acquisition of much information by the students. Yet even this Scotch lecturer, in some degree, aroused attention to anatomy, and as some of his preparations of the absorbents and of the heart, were deposited here, he may be said to have promoted our special study. Ramsey had adopted the notion that the bite of the rattlesnake was rendered innoxious by alkalies; an opinion sustained by Ramsey, of South Carolina, our old friend Dr. Mitchell, of New York, and by others who adopted the theory of the acidum pingue of De la Boe. Ramsey was adventurous enough to test the theory by an experiment on his own person; he allowed himself to be bitten—contrary to the usual action of that poison, he lingered in suffering, and finally died.

The making of anatomical preparations by the students of our rival medical schools, the occasional importation of specimens of singular value, from Europe, the broad and convincing fact that anatomy is the groundwork of a surgical and medical education, have caused the production of many precious displays of the art in these our own days. The great chirurgical achievements of the profession in this city, for the last twenty or thirty years, and the value of such evidence of patient study set forth by the teachers of our respective schools, have contributed largely to promote the strongest desire in the student to excel in such labors; and our leading men who assume the proper instruction of students, now very generally aim to be furnished with museums more or less ample.

A fact strikingly illustrative of the general desire to found museums of this nature as important adjuvants to the several branches of instruction in the medical art has been most happily evinced by the clinical faculty of our venerable and venerated New York Hospital. I have intimated that at an early day the salutary work was commenced; the public disasters of the times, however, arrested the undertaking, and it is only within the past few years that renewed efforts have been made for its accomplishment. The foundation is now laid; the work in progress, and it is conceded by all, promises to fulfill the highest expectations. An institution like the New York Hospital cannot fail to furnish materials of most instructive import for the great design; the quotidian laborers of that vast charity must supply specimens profitable to student and professor; her long list of surgical worthies recognized in Cheesman, Post, Buck, Watts, Watson, Parker, Van Buren, Sabine, give confidence that a museum of rare acquisition will ere long add further means to those already secured for elucidating clinical science. I am informed that the immediate conservators of this new collection are Dr. Agnew and Dr. Bayles.

I was not indifferent to this department of knowledge during my twenty years' services as Professor in the College of Physicians and Surgeons and in Rutgers' Medical College. The collection I formed during that period might claim notice from its extent and the nature

of the preparations. I had gifts, I purchased, and I prepared. The phenomena of death by lightning, by the drinking of cold water when overheated, cases of death by sun-stroke, instances of poison, yielded specimens of singular value; and as medical witness for nearly seven years in our courts of oyer and terminer, I was compelled in all criminal cases to ground my report on autopsic examinations. In obstetrical knowledge I had much of interest, in illustration of changes and infirmaties of the uterine system. This collection is now transfered to Geneva College, an institution of rising importance as a medical school.

From want of time, I shall forbear to dwell upon the collection of preparations illustrative of surgical cases at the hospital at Ward's Island under the direction of the able surgeon-in-chief, Prof. Carnochan, and the valuable collection of your surgeon, Dr. James R. Wood, of the Bellevue Hospital. The display of anatomical skill and surgical prowess which Dr. Wood's collection presents aided by Dr. Gouley and others, will in the course of the season enlarge your minds with principles confirmatory of what I have advanced in this lecture, and say more in behalf of the zeal and ability of your teacher than any words can express. Proofs of such skill yield more satisfaction by illustration and example than the most elaborate disquisitions.

I shall close these casual remarks on the history and progress of anatomical preparations with an observation or two on the publication which within a few days past has been generously distributed among you by the venerable Professor Valentine Mott. The devotion and life-long labors of this great practical surgeon and teacher are familiarly known to you all. Nothing but anatomy—surgical, topographical, minute anatomy—could ever have secured the mighty triumphs which enshrine his name. He had received a sound education, and had graduated M.D. at Columbia College in 1806; but he was not satisfied, and repaired to Europe for a residence of nearly three years. He became a pupil of Henry Cline, of Astley Cooper and of John Abernethy, and from this unadulterated fountain-head he derived that knowledge which has rendered him the contemporary and the legitimate successor of those master minds. As he had wisely repaired abroad for that information which at the beginning of his professional career could only in part be obtained at home, he became enriched with that wisdom which opportunity, labor, study, self-denial and perservance alone can give. His reputation as a student was announced to his countrymen ere his return among us. In the spring of 1810, he issued a notice of a course of surgical lectures which I, a young student had the gratification to hear, surrounded by many of the old practitioners of medicine. In the fall of 1810, he enlarged his plan, and the Professorship of Anatomy and Surgery, held by Dr. Wright Post, being divided, to Dr. Mott was assigned Surgery. Now it was that chirurgical science was taught as a distinct course in this country, and the talents and acquirements of this young professor demonstrated that his credentials in behalf of his high promotion justified the ample confidence the

trustees of Columbia College had cherished. You will thus see that for nearly half a century Dr. Mott has filled the responsible trust of instructor of chirurgical knowledge in this city. No other individual in the country has been thus employed for so long a period; no other teacher in the land has discharged the great duty with such vast benefits to the public weal. We must look to the army of youths whom he has indoctrinated to understand the merits of his prelections, and to the practical results which have flowed from the demonstrations of science and skill made manifest at his hand. I know that in the several institutions in which he has formed an integrant part he has been estimated at that consideration which superior merit alone attains; that the great art has been amplified by his inventive resources and his indomitable zeal; that the annals of American surgery are fertile of his great improvements; and that his name can never be lost while philosophy honors her votaries, and while humanity cherishes the sympathies of our common nature. I devoutly believe that the preeminent consequence with which surgery is regarded at the present day throughout our wide republic owes much to his unparalelled career as its master, and that its now elevated superstructure rests mainly upon the strong foundation of his professional skill.

With peculiar satisfaction, therefore, I refer you to the surgical museum of preparations, an account of which has been published by Dr. Mott. Its treasures are mainly the fruits of his operative qualifications, illustrated by the ripe labors of fifty years; the whole collection, we are told, is almost altogether composed of specimens made from his individual cases of practice. I shall forbear any enumeration at present. Well may Astley Cooper have exclaimed, when hearing of the bold and intrepid operations of his American rival, "He has performed more of the great operations than any other man living, or that ever did live!" That great museum is accessible to every one of you, in its appropriate hall in the Medical University of New York.

Gentlemen: I will trespass on your patience no longer. I have appealed to your intellectual nature to acquire science as the legitimate and effective substitute for the overthrow of empiricism; and fortified in that possession, practice, as its associate, will prove the exponent of your knowledge. The struggle of professional life to secure success, demands the why and the wherefore, and the mastery of such reasoning is your great reliance. I think I have said sufficient to impress you with the conviction that in repairing here, you have come to an institution rich in substantial materials for elucidation; among men who drink from a living spring of active practical knowledge; and such men, according to Locke, are the proper instructors. Here is no stagnant pool; the slough of despond is not visible. It is for you to catch the life and spirit of the place and of its actors. I am done.—*New York Jour. Med.*

Academy of Medicine (New York.)

[We extract from the Herald, the following account of the proceedings of the meeting of the New York Academy of Medicine, held on the 19th of January. Like most newspaper reports of medical discussions, it is in some particulars probably defective, but the main statements—at least those of Drs. Beales and Mott—appear to be authentic, having their signatures affixed:—]

A meeting of the Academy of Medicine, of unusual interest, took place at the University last night. There has been a rumor widely circulated through the city for some weeks past that a well-known citizen—Samuel S. Whitney, Esq.—came to his death by malpractice at the hands of one of the most extensive practising physicians of the city. This report of course created a lively and wide-spread excitement. The probabilities of its truthfulness have been warmly discussed on either side. It was expected that the highest medical authority of the city would give an authoritative expression of opinion upon the subject last night—so there was an unusually large attendance of the members of the Academy of Medicine.

At half-past eight o'clock, after some special business had been attended to, the matter of the death of Mr. Whitney was taken up. The President invited Dr. Horace Green to make his statement of the case before the post-mortem examination was read.

The doctor stated that this unfortunate case was first presented to his notice on the 25th of October last. Mr. Whitney came in with the rest of his patients, and made two calls at his office before he saw him. His assistant informed him that the gentleman had called, but would not wait for his turn, and wished to see him at once; to which he replied that his rule in such cases should be adhered to, and that unless in case of a lady, the patients should wait their turn. Mr. Whitney then came in in his turn, and entered his name on the doctor's book, as all his patients did. He stated to him that he had been in ill health for two or three years, and that for the last two months had felt quite a bad cough, and he also complained of his throat and chest. He stated also that some physician had examined him before, and had told him that his lungs were affected. He (Dr. Green) then made an examination of his chest by auscultation, in the presence of his assistant, Dr. Richards, who, as usual in all cases which he examined, made a note of it at the time. He found the chest thin, a little depression on the left thoracic wall; percussion gave a flat sound over all the upper portion of the left lung. On applying the ear to the chest a distinct humid rale or click was heard below the left clavicle in both inspiration and expiration. These symptoms, when accompanying the signs, were indicative, in his (Dr. Green's) experience, of the presence of tubercular softening. Mr. Whitney's throat appeared granulated and inflamed, and the left tonsil was slightly enlarged and ulcerated; the epiglottis was thickened, and its border whitened with a line of erosion. The doctor then gave an account of the several interviews which he had

with Mr. Whitney from the 26th of October to the 4th of December, and his treatment on these occasions—the application of a solution of nitrate of silver to the fossæ, epiglottis, and into the glottis, and the use of the "probang." The visits of the patient occurred at such long intervals that he found that the parts were not prepared for the introduction of the tube; but as Mr. Whitney had several times expressed a desire to have it used, he (Dr. Green) resolved, on the 6th of December, to make the attempt. The tube was, therefore, introduced and the nitrate of silver applied. Dr. Green then proceeded at length to detail the facts of the case, and stated that he had not seen Mr. Whitney from the 9th of December until he heard of the death. He was most willing that the matter should be discussed by the Academy, and he left the matter entirely in their hands. Dr. Green continued at some length.

The President then said that Dr. Foy should next be heard.

Dr. Foy rose and said: I was present on the occasion described by Dr. Horace Green, and saw the application made upon Mr. Whitney. Dr. Green has stated exactly the particulars of that occurrence. On introducing the tube into the throat of the decedent he made a sudden motion of the head and gave an expression of pain. The pain was not greater than I myself have felt upon having the uvula touched with nitrate of silver. The date of that visit is fixed upon my mind with certainty; so, also, is the size of the probang, for I remember noticing it very particularly, and telling Dr. Green that I could not get even a small probang, nor one thus curved, at the druggist's. Mr. Whitney left the office before I did, and left it not suffering any particular inconvenience.

STATEMENT OF DR. J. C. BEALES.

I find myself in a very disagreeable situation. It is the first time I was ever engaged in any controversy with any of my professional friends, and you have never known me to enter into any professional dispute of any kind before, I always have avoided it. Upon the present occasion I shall be forced to take a position antagonistic to Dr. Green, for which I have abundance of evidence; and I assure you it is not voluntarily assumed, but forced upon me.

In the statement of the case I am about to read to you I am sorry to say that there are some expressions in the commencement which are put in with very great reluctance, but owing to the different reports that have been circulated, I felt it necessary to insert them, that you may have a just comprehension of the state and feeling of the patient:—

Condition and Feeling of Mr. Whitney between his last treatment by Dr. Green and his death.—December 14, 1858. About one in the afternoon I was called to see Samuel S. Whitney; I found him surrounded by several members of his family, in a state of the most intense excitement, suffering, and terror; in answer to my inquiries as to what had happened, he answered: "Sit down, Beales, and I will tell you the truth; I was such a fool as to go to Dr. Green to

be operated upon, and the d—d villian has killed me." His countenance was pale and haggard, and had all the appearance of a man whose nervous system had received a severe shock; his breathing was occasionally irregular and almost spasmodic, coughing almost incessantly, and speaking with great difficulty and pain, in a hoarse and unnatural tone of voice; his skin was cold and clammy and covered with perspiration; the pulse was extremely frequent, feeble, irregular, and intermittent; he was excessively restless, not remaining in the same place more than a few minutes at a time; complaining of intense pain in the region of the larynx, shooting through to the cervical vertebræ, and down the course of the trachea to the chest; he kept grasping the larynx, and reiterating every few minutes that he was murdered; I endeavored to calm the excitement of the patient, and tried to examine his fauces and throat, which appeared in a state of great inflammation; I discovered no lesion, as, in fact, on account of the pain and terror of the patient, the examination was necessarily very imperfect, as he would scarcely allow the spoon to touch his tongue, and I concluded therefore to defer the examination till he should become more quiet; I gradually ascertained, partly from the family and partly from himself, that he had been several times to see Dr. Green; on the first occasion his tonsils had been amputated; at a subsequent occasion, ten or twelve days previously, (the exact dates were not told to the relator,) "a hollow tube had been passed into his lungs, and about a teaspoonful of solution of nitrate of silver had been injected into them by touching a spring at the top of the tube;" whether this was done more than once the relator does not recollect to have been stated; on the 14th of December Mr. Whitney breakfasted with his family, appearing to be in his usual health; he afterwards went to Dr. Green's office; "the doctor passed an instrument into his throat, and, finding some obstruction, he pushed the instrument with some force; he (Mr. W.) felt something give way, immediately experienced severe pain about the top of the windpipe, and told the doctor he had hurt him;" he returned home, informed the family of what had occurred, and I was called as before stated; one P. M. I saw him with the symptoms and in the state previously described; it was evident that, under these circumstances, the only indications that could be followed were to rally the patient's strength, to produce some reaction, and to moderate the local irritation in the fauces; to this effect I ordered him to be immediately put in bed, bottles of hot water to the feet, with sinapisms to the extremities and chest, and flaxseed poultices to the throat; a teaspoonful of chloric ether or volatile tincture of valerian in water occasionally, till reaction should be established, and a mixture composed as follows: ℞.—Ol. amygdal. dulc.; syrup. papav. alb.; mucilag. G. acag.; liquor potass; a desert spoonful to be slowly swallowed occasionally. For nourishment he was allowed arrowroot and flaxseed tea.

14th, 7 P. M. Is suffering severe pain, described to be in the larynx, down the course of the trachea to the chest, and round to the cervical vertebræ; pulse 112, feeble and irregular; still excessively restless; other symptoms are about the same; insisted on my re-

maining with him all night. ℞. Antimonial solut., S. morph., syrup. gummi, aq. destillat.; a desert spoonful every four hours; to inhale the vapour of infusion of flaxseed and poppy-heads.

15th, 3 A. M. They called me, as they observed the face to be swelling; I found extensive emphysema all round the neck, and partially in the face, rather more noticeable on the left side; he had continued exceedingly restless, scarcely dozing for a few minutes, breathing very irregular; pulse 106; urine scanty, very high colored and turbid. Continue the same remedies and nourishment.

1 P. M. Heat of surface more natural; scarcely any pain in the chest, emphysema very much increased round the throat and face, and extending down the chest; has not slept; has taken scarcely any nourishment on account of the pain in swallowing; could not continue the inhalations, although they rather relieved him temporarily. Anodine liniment to be applied to throat and chest.

8 P. M. Dr. Valentine Mott saw him in consultation with me. Is decidedly worse; emphysema very much increased; neck and face enormously swollen, it has extended all over the chest, but lower down on the right side; breathing somewhat labored; pulse very feeble, irregular, and 112; skin is again covered with clammy perspiration, and about the neck and chest of a purplish erysipelatus appearance; does not particularly complain of pain, except on talking or swallowing. Dr. Mott gave a very unfavorable prognosis. Continue anodine and take alternately a teaspoonful of ammoniated tincture of valerian.

16th, 6 A. M. Upon the whole has passed a more comfortable night; symptoms are all a shade better; the emphysema rather less in the face, but the throat and the chest are enormous, the mammæ resembling those of a stout nursing woman. Continue wine whey.

1 P. M. With Dr. Mott. The emphysema extends to Poupart's ligament on the right side; but only as low as the unbilicus on the left; cough less frequent, except when he swallows; pulse 108, and rather firmer. Same remedies and nourishment.

9 P. M. With Dr. Mott. Is not so well; emphysematous swelling increasing; cannot open his eyes till the air is carefully pressed out of the lids; chest and abdomen still more swollen; pulse more feeble, 122, although he has taken nourishment more freely. Same remedies.

17th. 6 A. M. Has slept more during the night, sometimes for nearly an hour at a time; has taken more nourishment, but there begins to be considerable mucous secretion, which interrupted his respiration and gives him great trouble to expectorate; pulse very irregular and feeble; the slightest movement increases its frequency; it averages about 108.

1 P. M. With Dr. Mott. There is no observable change in the symptoms, although he says he feels more comfortable; several attempts have been made from time to time to examine the fauces and adjacent parts, but the excessive swelling rendered them useless.

9 P. M. With Dr. Mott. There is again a slight lull in the symptoms, excepting the pulse, which is extremely irregular at 108—same remedies.

18th, 6 A. M. Has passed the best night since the attack; there is a decided improvement in all his symptoms; emphysema slightly subsiding; pulse 90; is rather more hopeful.

1 P. M. With Dr. Mott. We consider him decidedly improving; all the symptoms are milder; he is slightly flighty from the effects of the anodine.

9 A. M Is not so well again, without any other apparent cause than he would get up during my absence and sit for about an hour in a chair; the pulse is more frequent and irregular; the difficulty of swallowing is also evidently increasing, the attempt to do so bringing on coughing, partial strangulation, and some regurgitation of the fluids.

19th, 6 A. M. Passed a very bad night, principally owing to the great increase of the mucous secretion, that keeps him almost constantly coughing and expectorating, which he does with great difficulty and suffering; the pulse very frequent, feeble, and excessively irregular; take half the dose of the anodine at a time: (℞. Ammon. carbonat. grs. iv; emuls. amygd. dulc. dr. i, every four hours, in place of the tr. valerian ammoniat.); although it is certain that there is some serious lesion in the vicinity of the glottis, yet it is utterly impossible to ascertain the state of the parts; the emphysema has rather subsided about the upper part of the face, so that he can partially open his eyes.

1 P. M. With Dr. Mott. Has slightly rallied, but the mucous secretion is increasing; the cough more frequent, and difficulty of swallowing greater; bowels have not acted for three days; continue remedies; injection; give as much nourishment as possible.

9 P. M. All his symptoms much worse; pulse more feeble, 120—difficulty of swallowing, with the coughing and strangulation very much increased; consequently has not been able to take so much nourishment.

20th, 6 A. M. Has passed a very bad night; breathing laboured, and all the difficulties of swallowing, &c., increasing; the emphysema rapidly disappearing from the face and throat; abdomen distended and tympanitic; injection did not operate; a table-spoonful of castor oil.

1 P. M. With Dr. Mott. All the symptoms gradually becoming more serious.

10 P. M. Is very much worse in every respect; respiration excessively laboured; the slightest attempt to dose threatens suffocation from the accumulation of mucus; can with difficulty be induced to swallow; the oil operated twice, and he was excessively exhausted; pulse extremely feeble and irregular, 126; he is evidently sinking.

21st, 7 A. M. During the night he became rapidly worse; did not swallow after 2 A. M., and died rather suddenly at 8 A. M., partly from exhaustion and partly by asphyxia.

Note.—A number of trifling circumstances, such as the varying appearance of the urine, the continual slight changes in the symp-

toms, &c., as not throwing additional light on the case, have been omitted, in order not to make the statement too tedious.

J. C. BEALES, M.D.

As far as relates to this case, from the time I was called in, it is a faithful narrative. VALENTINE MOTT, M. D.

I certify that this is a faithful copy of the original.
New York, Jan. 18, 1859. J. C. BEALES, M.D.

POST-MORTEM OF SAMUEL S. WHITNEY.

New York, Dec. 22, 1858.

Thirty hours after death nothing peculiar in the appearance of the body. Rigor mortis quite moderate. On making an incision from under the chin, in the mesial line of the sternum, it was remarked that the anterior projection of the thyroid cartilage was more than ordinary. Directly as the knife divided the deep cervical fascia on the left side of the thyroid cartilage, pus issued out—a little further division opened into a cavity, containing pus about the size of a large hen's egg, and extending a little in front of the pharynx, and downward behind and below the thyroid cartilage. At the upper and posterior part of this abscess there was an opening into the pharynx, large enough to admit the end of the forefinger. This abscess was lined by a large quantity of destroyed filamentous tissue, hanging from different parts of it like wetted tow. The entrance into the œsophagus immediately below this was perfectly sound, internally and externally. The larynx was now laid open from behind, and, at the first glimpse, a red point about the size and shape of a grain of wheat, on the left side, a little below the left chorda vocalis, and running longitudinally, led us to exclaim, there is the point of laceration of the mucous membrane, by which the air has escaped into the cellular tissue to constitute the emphysema. On close inspection, and wiping the part with a sponge, no abrasion or aperture could be discovered. Every other part of the larynx and trachea, as far as removed, presented on its internal surface a perfectly normal appearance. Indeed, we all remarked that we had never seen a larynx and trachea more natural and healthy. We next concluded to have a look at the bronchi and lungs. Perhaps about an inch above the division of the trachea, the most beautiful vermillion redness that we ever saw on a mucous surface commenced and extended into each bronchus, but greatest in the left, and extended down each lung. Over this peculiar redness there was a cloudy shade, which vanished after a short exposure to the air. On opening the pleura, the upper lobe of the left side, at first glance, seemed covered with white, thick pus. But on close examination, it proved to be soft, strumous-like fibrin, easily rubbed off. This, on the side and posterior part, connected that lobe in patches to the pleura costalis. These imperfect adhesions were easily broken down with the fingers. The whole of the upper part of this lobe was very

red and solid, hepatized. Just at the root, or at the commencement of the bronchial ramifications, there was an open cavity, about the size of a small black walnut, of a reddish-brown color, and irregular billows surface, as though a slough had separated. At the upper and anterior part of this cavity there was a small opening through both pleuræ. This lobe was cut into in different directions, but no tubercles could be found. The lower lobe was perfectly healthy. The redness of the mucous membrane of the right bronchus extended to the lung of that side, but the three lobes were perfectly normal. There were no old adhesions on either side of the cavity of the chest. Some little appearance of the emphysema remained.

VALENTINE MOTT, M. D.,
J. C. BEALES, M. D.,
ALEX'R B. MOTT, M. D.

Dr. Beales said: During the number of years that I have attended Mr. Whitney's family, I have not known Mr. Samuel Whitney to be seriously ill, so as to be confined to his bed; but he has for a long time been subject to various derangements of the digestive organs, such as want of appetite, torpidity of the bowels, deficiency of the bilious secretions, and occasionally a bronchial cough. For these I have frequently prescribed for him; but during the whole or greater part of the last year (as I have been informed by the family) he placed himself under the care of a homœopathic physician, so that, with two or three trifling exceptions, I was not called on to prescribe for him until the present occurrence. Toward the end of October his sister informed me that her brother was very low-spirited and depressed, as some physician had informed him his lungs were very much affected. He wanted me therefore to examine him, but did not want me to know that he had consulted any other physician. I was not told who it was, nor do I know to this day, although I now presume it to have been Dr. Green. Sir, I wish to state that I appreciate the stethoscope as highly as most men; I believe it, as most others do, one of the greatest discoveries in our profession, but I frankly confess that I do not believe in its infallibility, even aided by percussion. I do not believe that any man can at all times discover one or two, nor even a few tubercles, scattered about the upper lobes of the lungs. I am sure that every man, if he would frankly tell the truth, would admit that he had occasionally been mistaken. For myself, I do not pretend to any extraordinary skill with this instrument, but, independent of my private practice, I have been for fifteen years examiner for various life-insurance companies, and therefore I constantly make use of it, and ought to know something about it. Now, under these circumstances, well knowing the opinion of the other physician, I examined Mr. W. with all the care and accuracy of which I am capable; I declared to him that I could not discover any tubercles in his lungs, and that I did not believe that any existed. [No notes of the examination.] Now, sir, on turning to the report of the post-mortem examination, it will be seen that a "cavity" was found, but not a single tubercle.

I will not, of course, assert that such a thing as a tuberculous cavity never exists without the presence of other tubercles, but I do say, that it is a most rare and exceptional circumstance; but I wish to make a few remarks on this "cavity." Was this a tuberculous cavity? It neither contained any kind of fluid, nor was it lined with lymph, nor the slighest appearance of false membrane, nor were there any remains of tuberculous deposit, and I at least have never seen a tuberculous cavity similar to it—in fact, although that word was used in the report as probably most readily occurring—it could scarcely be justly so called, it was rather a shallow depression or scooping out of the actual apex or superfices of the lung; its surface was not like that of a "cavity," but rough and irregular, and had that peculiar appearance that all present remarked it looked as though a slough had separated. Communicating with it was a perforation in the pleura sufficiently large to admit the little finger of the gentleman who had operated; all other appearances about the lung were of the most recent disease, the hepatization was in its earliest stage, and the adhesions spoken of were so recent that the folds of the pleura were, more properly speaking, glued together than adhered. We did not discover the slightest sign of chronic disease in or about the lung; and so striking was this fact, that Dr. Mott told the family, after the post-mortem examination, that we had not seen any diseases that might not have been produced within a week. But Dr. Mott is here, to speak for himself. Dr. Green says that the epiglottis was thickened and its border whitened with a line of erosions. At the post-mortem, this part was very minutely and carefully examined, and found to be extraordinarily healthy and free from the slightest vestige of disease. Under all these circumstances, I am forced to believe that Dr. Green erred in his diagnosis, and that these various operations were unnecessary and uncalled for. I do not say that the operating of tubing caused the disease in the lung, because I confess myself ignorant of the effects of nitrate of silver on the substance of the lungs; but for the operation itself, I do not hesitate to express my conviction that it is at all times attended with extreme peril and risk of the patient's life. I have never heard of or seen a single case of phthisis where it has effected a cure, and therefore I believe it to be perfectly unjustifiable. I believe that a slough or eschar was formed at the apex of the lung, involving the pleura, and which, at the time of the unfortunate occurrence, became separated by the violent exertions and spasmodic coughing—the air percolated into the cellular substance, and produced the emphysema which formed so prominent a symptom. I will now leave this part of the case, and go on to that which was, after all, undoubtedly the immediate cause of the death of the patient. I mean the lesion of the pharynx. By referring once more to the post-mortem examination, it will be seen that there was a lacerated opening in the pharynx communicating with a large abscess. I have heard it rumored—and indeed it has been stated in the public papers, especially in an article in the *Tribune*, which is evidently from a suspicious source—that this abscess was chronic. Insinuations were

made against Dr. Mott and myself in regard to it. If, sir, the friends of Dr. Green have given currency to this idea, or intend in any way to suggest it—then has the Dr. ample reason to say, "Defend me from my friends." It appears by his own statement, that for two months previously to his death, Mr. W. was under the professional care of Dr. Green—for my own part, I solemnly declare, I have never prescribed for nor heard him complain of his throat. Early in October the doctor cut out one of the tonsils. Did the chronic abscess then exist? If so, how was it that the doctor did not discover it? He several times applied the sponge and probang—did the abscess then exist? On the 8th of December, Dr. Green states that he passed the tube down the tracheæ. This, at all events, whatever we may think of the operation itself, requires a careful observation of the parts; did the abscess then exist, and the doctor not discover it? But, sir, on the very day of the last unfortunate operation, Dr. G. was showing to Dr. Foy how he applied the sponge to the larynx and showed why it only entered the pharynx—of course the organs were closely observed—how was it that the doctor did not diagnose this chronic abscess? Why, sir, the reason that Dr. G. did not see this chronic abscess, was because it did not exist.

Sir, I do not believe that among all those who are now listening to me, there are two opinions. At all events, to my mind, the evidence is irresistable, that in the last unfortunate operation, on the 14th of December, that the pharynx was accidentally lacerated by the probang; the first effects, as we have seen, were excessive irritation of the parts, and a severe shock, increased no doubt by the nervous temperament of the patient, and his conviction that the injury was fatal. Afterwards, doubtless, portions of the various foreign bodies he attempted to swallow, food and medicine, were forced into the wound. After three or four days a sloughy abscess began to be formed, which, gradually increasing in size, formed a mechanical obstruction to swallowing; by pressure on the adjoining parts, prevented the epiglottis from properly closing, and produced the strangulation and regurgitation which we have noticed, till at length the unfortunate patient sank from exhaustion and asphyxia. I wish now, sir, with your kind permission, to make a few remarks with respect to the post-mortem examination. I perceive by statements in the public papers, the source of which can easily be understood, that we are censured for not having Dr. Green present. I need not say that, as the case progressed, the excitement and feeling in the family did not diminish. I do not think that on this point I have the right to judge Dr. Green; he doubtless did what he thought right in the matter; but had he, by sending inquiries or showing any sympathy with the misfortunes of the family, it would have offered an occasion to Dr. Mott and myself to have introduced him; that he did not so act, was repeatedly remarked by many of the family. Now, under these circumstances, it was no pleasant thing to ask permission of the family, and I frankly allow we did not. But, for myself, I solemnly declare that I went to that examination without the slightest idea of criminating Dr. Green, but

with the earnest desire to ascertain the nature and extent of the injury. But let me ask, what do these insinuations mean? I will tell you how the post-mortem examination was arranged; I asked Dr. Mott who he would wish to perform it; he replied, his son, Dr. Alexander, and on the day of that operation I was introduced and spoke to that gentleman for the first time in my life. The insinuations to which I have alluded either mean that we were not competent for the examination, (if so, let the truth be told,) or that the examination or report was distorted to meet particular views. On this point I shall merely remark, that Dr. Alexander Mott has never, till to-night, heard me say a word as to my views of the case. I do not know his. We have never interchanged a word on the subject. Both he and his father hold such positions in the profession and society, as ought to place them beyond such calumnies. As for myself, those who know me, sir, will not, I am proud to believe, imagine me capable of misrepresenting solemn facts, for any purpose whatever; and this is all, sir, I think it needful to say in answer to these unmerited and disgraceful innuendoes.

Dr. Valentine Mott followed, strongly substantiating the post-mortem examination, which, he said, was prepared by himself, and controverting Dr. Green's theory of the case.

After him, there ensued a spirited discussion between Drs. Mott and Beals on one side, and Drs. Green and Foy on the other.

Dr. James Wood moved that the matter be referred to a committee. An exciting debate ensued upon that and other motions. There was cheering and hissing. The commotion and debate continued unabated until 11½ P. M., when the whole subject was tabled.—*Med. News.*

John Dawson, M.D.:

Dear Sir—I have received a copy of the following interesting paper on transfusion, from the author, a scientific gentleman of New Orleans, with whom I formed a pleasant acquaintance during a visit made to that city last spring. If agreeable, I should be pleased to see it in the next number of your able Journal. The patient was his only sister. It saved her life in October last, while the epidemic, yellow fever, was yet prevailing with such sad and fatal results. He writes me that she remains entirely well.

Very respectfully, yours, &c.,

WILLIAM M. AWL.

Columbus, O., January, 1859.

Transfusion in Yellow Fever—A Successful Case. By N. B. Benedict, M.D., of New Orleans.

Messrs. Editors:—In consequence of the pressure of indispensable engagements, I am able to furnish you only a brief outline of the

circumstances attending the case of transfusion which was successfully practiced in this city, on the 25th of October last.

The patient, Miss J. B., was a young lady whose life, until early womanhood, was passed in north-western New York, whence she removed, in 1845, to Mississippi, and thence, in 1854, to New Orleans, where she has constantly resided during the past four years: never having suffered any serious illness within the last fourteen years.

On Monday, October 11th, she was exposed to a drenching rain, but felt no alarm at the sensations which followed, (supposing them to be merely those of a bad cold,) until the evening of the following day, when she was seized, at 9 o'clock, with a violent chill. The symptoms of yellow fever—the pains in the head, the back, and the limbs, the flushed face, the fiery-red eye, and the rapid pulse—could not have been more characteristic than in this case. It proved unusually obstinate—the treatment exerting little influence upon any of the symptoms, during the first stage of the disease. My near relationship to the patient induced me, on the third day, to request the assistance of Dr. C. B. White. On the 5th day the counsel of Dr. Wm. E. Kennedy was requested, and both those gentlemen continued their attendance, daily, throughout the course of the disease. On the fifth day, there was some bleeding from the mouth; but the flow was moderate, and occasioned no alarm. On the sixth, it contiued, but was so slight as to be regarded as marking a favorable tendency. The average pulse, on these two days, was 86—ranging from 80 to 92. On the morning of the seventh day, the eye presented an appearance as if a drop of serum had collected in the outer canthus; the bleeding from the mouth had increased, and the appearance of the napkins employed to absorb the blood, agitated her excessively; but at the mid-day visit, although the hemorrhage was unabated, yet so much had the complexion improved, that the impression on the minds of the medical attendants was, upon the whole, favorable. The hemorrhage increased until a late hour in the evening, being aggravated by the uncontrollable perseverance of the patient in wiping away the clots. It suffered a slight check during the night but returned on the following morning. Throughout the eighth, and most of the ninth day, it continued without abatement; and her efforts to clear the mouth by hawking, rinsing, spitting, and wiping, not only increased the flow, but occasioned great fatigue. To prevent these efforts, except by a resort to unjustifiable force, was impossible. The effects of such protracted hemorrhage were apparent in the blanched complexion, the colorless lips, tongue and gums, and the pinched features; and serious alarm began to be felt at a loss which resisted all the measures employed to restrain it. Some estimate may be formed of the quantity lost, from the fact that, in the single night of the eighth day, the blood diluted with saliva, saturated two sheets, so that no part of them remained unsoiled. Added to all this was the hemorrhagic tendency manifested by other mucous surfaces besides those of the mouth, during these two days. Towards the close of the ninth day, the hemorrhage from the mouth was several times interrupted for short intervals; but owing to the discomfort caused by

the clots, she provoked its return by removing them. About 3 o'clock, P. M, she had a sudden imperfect syncope. Thence forward the bleeding became very slight, and although nourishment and stimulants were retained, the complexion was much blanched, and the mental condition was that of utter despair as to recovery. The rate of the pulse was steadily accelerated; the average for the seventh day being 98, for the eighth and ninth days 104, for the tenth day 115, and for the eleventh and twelfth days 120.

On the tenth day there was much complaint of soreness of the throat the tongue, and the inner surface of the lips (which were found to be invaded by small superficial ulcers); of severe pains in the sides of the head, at the tops of the ears; of morbid sensations—at one time, as if ashes filled the throat—at another, as if the feet were much more elevated than the head; and there was uncontrollable desire to talk of affairs and of interests the most foreign to a sick room; yet, all was perfectly rational. In the course of this night, the hemorrhage ceased, and did not again return. The eleventh and twelfth days were characterized by a continuance of those nervous vagaries, and by the increased frequency of the pulse to 120.

At 3 o'clock, in the morning of the thirteenth day, she had profuse perspiration, and a paroxysm of hysterical weeping. The general surface, as well as the extremities, were cold and clymmy to the touch; the pulse so feeble and so rapid as not to be counted; and the restlessness, and desire to toss about the limbs, was excessive. Under the use of stimulants, this condition was relieved, the natural warmth was restored, and she fell asleep. At 7, A. M., the pulse had receded to 112, and was regular though feeble; and nearly every portion of the surface—the limbs as well as the trunk—was found to be covered with sudamina. At 10½ A. M., the apparent amendment was so marked, that the medical attendants were more hopeful than on any previous occasion. The pulse had fallan to 108. From the sixth day to this time, nourishment and stimulants had been taken in quantities such as to encourage the belief that the terrible waste of blood would be repaired by the ordinary process of assimilation. At 11½ o'clock, she awoke from a quiet sleep, complained of strange and bad sensations, and said she thought she must be dying. She presently turned upon her side, and was suddenly seized with vomiting, which continued throughout the day and part of the following night. Nothing was retained by the stomach, with the single exception of brandy, and even that only occasionally. After mid-day, an enema of mustard and water was given, but did not return. It was followed by other enemata of warm water, with no result except distention of the abdomen. The sinking and prostration were excessive; the pulse was scarce perceptible, yet its frequency was not found, at any time, to exceed 108; and she exhibited a constant tendency to lapse into stertorious slumber. Over the whole region of the aorta was distinctly heard the bellows-sound so characteristic of anemia. Injections of beef-tea, brandy, and carbonate of ammonia were repeatedly given, only to be rejected; but

the action of the bowels seemed limited to the rectum. At every movement of the body, distressing hiccough occurred, convulsing the whole frame, and lasting on each occasion, about a minute. A Sinapism applied to the spine was succeeded by arrest of the vomiting, at 10 P. M., but there was no other improvement throughout the night of the thirteenth day.

On the morning of the fourteenth day, a stimulating enema was followed by discharges resembling coffee grounds, and by relief of the distention, but there was no amelioration of the symptoms which steadily tended towards the fatal close. The mortal restlessness increased, the pulse became but the merest flutter, and was, much of the time, inappreciable, and its number could not be ascertained.

At the morning consultation, Drs. Kennedy and White declared their conviction that she could not possibly survive for more than three or four hours. I then said, "It is an old saying, that 'drowning men catch at straws.' For the past twenty-four hours, I have been in torture with the thought of such a straw, and I can not refrain from naming it: I mean transfusion. These fatal symptoms being occasioned by the loss of the last few ounces of blood, I can not persuade myself that it is not a serious duty to inject into her veins a few ounces, of fresh, healthy, living blood, as nearly as possible identical with that which she has lost." Both those gentlemen promptly embraced the suggestion that it was the only remedy that could save her life; but thought it impracticable for want of persons experienced in the operatian, and a suitable apparatus. Having, in 1853, prepared a report on transfusion, (which was read before the State Medical Society, and a part of which was printed in *The New Orleans Medical and Surgical Journal* for that year.) I had become imbued with the conviction that the operation is one which, on certain occasions, can not be withheld without criminality; and I had obtained from England the apparatus for the purpose, which was approved by the father of transfusion, Dr. James Blundell, formerly "Lecturer on Midwifery and Physiology, at Guy's Hospital" The possession of such an instrument at once gave form to the proposition, and it was determined that if a person could be found who would undertake the most difficult and delicate part of the process—that, namely, of preparing the vein of the patient—the operation should be attempted. A little past the hour of noon, there were assembled at my house, Drs. Wm. E. Kennedy, C. B. White, C. C. Beard, D. W. Brickell, and L. Greenleaf. The apparatus was examined, its use described and illustrated, and every part of the process was fully explained and dicussed. The task of preparing the vein was undertaken by Prof. C. C. Beard. An incision about two inches in length was made over the median vein in the left arm. A director was passed beneath the vessel, near the lower part of the incision, in order that it might be held under perfect control, and the loss of any blood from its distal extremity be prevented. An incision was then made into the vein, immediately beyond the director, to receive the beak of the syringe. It is due to Prof. Beard to say that nothing could exceed the skill and steadiness

with which this operation was performed. The blood was obtained by the assistance of Dr. Greenleaf, from the arm of a young gentleman who exhibits a remarkable example of perfect health, and who had experienced yellow fever during the epidemic of 1853.

It was my purpose to use the apparatus myself; but as the moment approached, my near relationship to the patient made me distrust my firmness, and I requested Prof. Brickell to take my place. He consented to do so, but substituted for the syringe belonging to the apparatus, one of simpler construction, into the beak of which he absorbed the blood as it flowed into the receiver of the transfuser. By inverting the syringe and pushing upward the piston, the last bubble of air was expelled. The beak was then introduced, by Dr. White, into the orifice of the exposed vein of the patient, and Prof. Brickell, with consummate care, passed the blood into her arm, before it had time to cool or even to repose for more than a few seconds. All the apparatus employed was immersed in warm water, or wrapped with heated cloths, so as to prevent any reduction of the temperature of the transfused blood. The operation was commenced at a few minutes before 1 o'clock, and was finished safely and satisfactorily, in all respects, a few minutes past that hour.

Another person who was constantly with her, writing to a distant friend, used the following language: "There is no doubt that Death had begun his work before this took place. Her extremities were cold; she swallowed with difficulty; her nose had the pinched look of death; her lips were depressed and bloodless, covered with a yellow, dry, parched skin; and her distress was very great. The effect of the transfusion was immediately apparent in the calming of the nervous system. The next morning, her lips were full and red; I picked off the yellow parched skin, and found them as natural as health; and she had no subsequent bad feelings except dryness of the mouth, and stiffness of the muscles of the throat. She was like a new creature, and was saved."

I will merely add to this statement, that the pulse, which at half past 12, under the influence of medical excitement, became once more appreciable, numbered 136, and immediately before the operation, 125; at its termination it was 120; and three hours later it remained the same, but had acquired more fullness and strength. The voice recovered its natural tone, the face acquired color, the extremities grew warm, all nausea and hiccough ceased, and ordinary drinks were perfectly retained. From the time mentioned, to the present hour, her recovery has been uninterrupted. Her health has long since ceased to be a subject of any anxiety.

It was not doubted at the time of the operation, that the quantity of blood injected was equal to three and a half ounces. It was afterwards ascertained, by accurate measurement of the syringe, that its capacity was not quite equal to two and a half ounces. Small as the quantity was, it yet sufficed to turn the scale in her favor.

I have thus, Messrs. Editors, endeavored to give a plain statement of the facts as they occurred. I have been the more particular in these details, because I am informed that an erroneous impression has, to

some extent, prevailed that I was the sole operator. The error is attributable to ignorance of the complicated nature of the operation, and to misconception of the flattering notices which appeared in a daily paper of this city, shortly after the event. Transfusion was not adopted without the consent of men who are among the first in the profession; and I venture the assertion that it never will be safely performed, on the human subject, without the assistance of four medical gentlemen at least. Nothing could be more mortifying to me than the acquiring of professional distinction by any means but such as are recognized by the medical profession as legitimate, unless it should be the acquiring of distinction which is unmerited. I trust that you will appreciate, therefore, the propriety of these explanations being made in your journal, inasmuch as justice required details which are improper in a newspaper.

Were the true nature and statistical results of the operation of transfusion generally known, its performance would be demanded in many cases which are now consigned to a remediless doom. There is no fear that it will ever come to be employed as one of the common remedies. It is applicable to no pathological condition, save that which is commonly called "collapse," induced by hemorrhage, by certain exhausting discharges, or by utter inability to receive or retain nutriment; and the only transfusion now sanctioned, either by physiology or by common sense, is that of *human venous blood into human veins, identical, as nearly as possible, with that which has been lost, and in quantity just sufficient to arrest the tendency towards death.* Prior to the year 1853, the total number of recorded cases, practiced under these conditions and restrictions, amounted to twenty-one. Of these, but two died—or, less than one case in ten!

If I have any merit in this affair, it is, that I have been, for many years, the earnest advocate of transfusion, in those cases where alone it is proper; that I have labored to make it binding upon the consciences of medical men to practice transfusion whenever it is justifiable; that I have approved my faith in it by submitting to it, as the first instance which I am apprised of in America, a person whose life is as dear to me as my own; and that before it was attempted I seriously declared to those professional friends, heretofore named, my willingness to take upon myself all the odium that might attach to a failure. I feel that I can never sufficiently thank those gentlemen for what they did for me in this the bitterest trial of my life; and that they have thus laid upon me new obligations of fidelity to the principles of our noble profession.

New Orleans, November 27th, 1858.

Ecrasuer in New York Hospital.

So much is said now-a-days about the ecrasuer, it will perhaps not be inappropriate to commence with a short account of the use of this instrument.

There have been but two operations performed in this institution with this instrument—one was upon a case of hemorrhoids, and the other for amputation of the penis. It was first used in the case of hemorrhoids by Dr. Van Buren. The patient, a lady 49 years of age, had sufferred more or less from this troublesome difficulty for a period of ten years, during the whole of which time she obtained very little relief from the palliative means usully made use of in such cases. On her admission to the hospital, October 12, 1857, she had four tumors around the verge of the anus; the two largest were about the size of a large hickory nut, and the other two as large as the end of the little finger. She being very desirous that something should be done for her permanent relief, an operation was determined upon and performed with this instrument four days after admission. The two largest tumors were only removed by the ecrasuer, the other smaller ones being ligated in the usual way. The opperation occupied about fifteen or twenty minutes, and but a small quantity of blood was lost. The parts healed kindly, the ligated portions separating in good time, and she left the hospital entirely cured on the 4th of November following

The next case occurred in the service of Dr. Halstead; amputation of the penis for warty degeneration of the glans; suspected to be of a malignant character. The patient was 16 years of age, with a fair amount of bodily vigor. The diseased mass occupied the whole of the glans, spreading out into excrescences which were equal in size to a couple of English walnuts. The disease first made its appearance five months previous to admission, a short time after impure connection, and kept gradually increasing until it had attained the size already mentioned. Its growth was, without doubt, very much accelerated by the want of proper attention to cleanliness. Various means were resorted to while in the hospital to reduce the diseased mass, but with no good result attending; finally, a consultation of the attending surgeons was held, and it was deemed expedient to amputate. This was accordingly done.

The operation, which occupied about twenty minutes, proved a very successful one as far as the amputation was concerned, but a considerable amount of hemorrhage followed from the open mouths of several vessels, one of which was the arteria dorsalis penis. The application of several ligatures was rendered necessary. The parts healed nicely, the process being much accelerated by the favorable shape of the stump following the action of the instrument, the integument tending to turn inwards, leaving but a small extent of raw surface to heal by granulation. The meatus urinarius was kept pervious by the occasional introduction of an instrument, and before he left a large sized cetheter could easily be passed. He was discharged cured a couple of weeks after amputation. No sloughing followed

in either case. The instrument used in both cases was *Chassaignac's*, manufactured by *Leur of Paris*.

The last case tends very decidedly to disprove one of the advantages claimed for the écraseur, viz., arrest of hemorrhage. A considerable amount of blood was lost, notwithstanding due deliberation was employed in the lacerating process.

This fact is also illustrated by a case that occurred in the practice of Dr. Van Buren, at the St. Vincent's Hospital, after amputation of the cervix uteri by means of this instrument, in which thirty-five minutes were employed in the operation. An artery required the ligature in consequence of its continuing to give forth blood.

I have learned that Dr. Isaacs, of Brooklyn, performed subsequently a similar operation upon the cervix uteri with a good result.

The surgeons generally look upon this instrument as useful in cases where amputation has to be resorted to upon parts which are not easily accessible to other instruments, and where, under ordinary circumstances, profuse hemorrhage is expected. They, in common with every one who can amputate with dexterity and neatness, by no means sanction the use made of it by some of the French surgeons of the present day; for instance, as an instrument for amputation of the limbs. With all the advantages claimed for it in such cases, they are by no means sufficient to tempt the operator to throw aside his catlin and resort to a practice which, at the best, is a very barbarous one.—*New York Journal.*

PART THIRD.

BIBLIOGRAPHICAL NOTICES AND REVIEWS.

Mind and Matter: Or a Series of Physiological Inquiries, in a Series of Essays, intended to illustrate the Mutual Relations of the Physical Organization and the Mental Faculties. By SIR BENJ. BRODIE, Bart., D.C.L., Vice President of the Royal Society. With additional notes by an American editor: New York—Samuel S. & Wm. Wood, 390, Broadway. 1858.

The author of this little volume, it is well known, has an enviable reputation. With the qualifications of a very able physician, he unites very rare powers of intellect. No man in the medical profession is looked upon as possessing more of discrimination and strength—qualities of mind indispensable to the medical man, who, of all others, has the most to do in unraveling the mysteries of

nature, and understanding the influences of Sociology. Thus endowed, the author is certainly calculated to inspire, with reference to the inquiries he has undertaken, although proverbially difficult, a large amount of expectation. What, says the reader, has Sir Benj. Brodie to say on this most difficult of all human inquiries—mind and matter. His professional avocations have certainly afforded him opportunities for the investigation; and he has now got it into his head that he is possessed of something worth knowing, and he is ready to give the world the benefit of his labors. In short, from just such a mind as that of Sir Benj. Brodie we might reasonably expect light to be thrown into crevices and across chasms not hitherto illuminated. The book before us, therefore, we willingly acknowledge, presents the conflict of a great mind with a great subject. We have, however, said enough of expectations, and shall now proceed to give a very brief notice of the author's performance:

An examination of the work impresses us with the purity of the style in which it is written. Sir Benj. seems to have ideas, and he expresses them in very few words. He has adopted the dialogue form as the best adapted to the subject. The work, therefore consists of six Dialogues, with "Additional notes by an American Editor."

The first Dialogue embraces rather a wide range of inquiries. We shall only, however, notice a few of its more important points. The first question brought to the attention of the reader is *Mental Labor*. This, in our judgment, is disposed of very cleverly. Most persons regard labor of all kinds as one of the curses entailed upon the race, and, as a consequence, a period is looked forward to when such troubles are to cease. Of a great many this expected period is the earthly heaven, and all energies are employed to bring it about. The merchant, the farmer, and the mechanic, work day and night to obtain the means which will render work unnecessary. All classes strive to get what they call a competency for children—means enough to make it optional with the child as it regards industrial pursuits. Is such a course philosophical? Is it grounded in the nature of things? Is it calculated to make life agreeable, to promote a desire to live? The author before us thinks not—and we fully agree with him. Mental exertion is just as indispensable to health as physical, and more bodily disease results from the neglect of it than from the neglect of physical. The brain was made to do a certain amount of work every day, and this it must do to avoid

who, of all others, has the most to do in unraveling the mysteries of

atrophy or disintegration. "We must do something to kill time," is a vulgar phraze that expresses a great deal. Many business men, under the impression that they will be more happy, abandon their pursuits, and then die of *ennui*, or contract hypochondriasis, or some mental or physical trouble that renders life a burthen.

The idea, therefore, entertained by so many of finding enjoyment in a life of leisure, is, without doubt, erroneous. As long as mental capacity is present something must be provided upon which it may be daily exerted. This should be carried to the extent of producing the sensation of tiredness. Sir Walter Scott found himself wearied daily with five hours labor. Bacon worked only about four hours daily on the Novum Organum. Cuvier worked but about four hours. The labor of Bacon was the most severe At every step something new was developed, which, in its multiplicity of relations, had to be considered. After he became acquainted with the zöological type, Cuvier had much in the way of mere repetition. Although possessed of a very uncommon amount of information for a man in his day, Scott plied mostly his powers of imagination. In short, the first operated mostly with logic: the second with special senses; the last with the imagination. Yet it seems either of them were incapable of six hours daily mental toil; or rather were not capable of *enjoying* six hours of daily bliss. The period of repose from intellectual pursuits is one of enjoyment. Here the stress is taken off, and in its place extacies and felicities, without the consciousness of effort, float through the mind in rapid succession.

What we have said relate to minds of the first order—minds made for understanding something of what surround them. The great mass of mankind think but little. They secure repose by bodily fatigue. They seem to live for the animal, calling into play only so much in the way of thought as is necessary for the limited sphere of their operations. With this want of cultivation, they become an easy prey of prejudice, passion and fanciful analogies. There would be no epidemics of fanaticism if the cultivation of the observing and reasoning faculties were general. This, however, is never to be expected. It implies a higher organization of the nervous system than is found to obtain. But suppose the masses could be organically leveled up to the right point, the pursuits which necessarily obtain in a good order of civilization, are far from being favorable to either accuracy or intensity of thought.

We have often been struck with the ease, for example, with which

a certain amount of work every day, and this it must do to avoid

well educated men are ensnared by the traps which the medical mountebank sets for them. But when we reflect that the mathematician is led to be over-credulous from the nature of his pursuits, and that the clergyman operates only on the faculty of credulity, and that poets and novel writers deal daily with the imagination, we may cease to wonder why it is, that a science which requires, in order to be appreciated, close observation and accurate powers of reasoning, fails to be distinguished from the most transparent humbugs.

Many persons of leisure, who find time very heavy on their hands, try to pass it off by reading. This is not apt to be successful: mere reading without an object in view disposes to irritation rather than tranquility.

The public mind of the present age has dogmatized itself in favor of general education, and it is ready to deal out censure in some form or other, on all those who do not subscribe and support, to the fullest extent, the measure. It seems to be the notion that all classes of society should be put through the schoolmaster's mill—that all should have a good knowledge of their mother tongue of the dead languages, of figures as far as mathematics; and as much in the way of modern languages as time will permit.

In regard to the propriety of trying to make scholars out of all the children of a community, doubts are by many entertained. Supposing it practicable, would it contribute really to enjoyment? Would it have a tendency to make the masses more peaceable, more industrious, or more inclined to exercise proper veneration toward rulers and established institutions. Where in our own country have we the most fanaticism? In what region has spiritualism and table-turning had the most votaries? Or, is it a mere coincidence, that those portions of our country which are able to boast of having the best educational institutions are afflicted with the greatest number of enthusiasts and imposters.

Is it philosophical to subject all the children of a community to the same routine of studies? This would imply that they are alike in mental constitution, and that they will develop alike, than which nothing is further from the truth. Common minds might, perhaps, be elevated by such a course; the better order in all probability dwarfed. Sir Humphrey Davy, in a letter addressed to his mother, in regard to his schoolmaster, after he had settled in London, says: "I consider it as fortunate that I was left much to myself as a child, and put on no particular plan of study, and that I enjoyed much idleness at Mr. Coryton's school. I perhaps owe to

these little circumstances the little talents I have, and their peculiar application. What I am, I made myself. I say this without vanity, and in pure simplicity of heart." Thus wrote the man who, of all others, gave to chemistry the greatest impulse. The earths and alkalies yielded their greatest secret to his genius. In the medical profession, John Hunter, if measured by his contributions to science, or the originality and comprehensiveness of his views, is the tallest man. Those who are familiar with his history are aware that he knew scarcely anything of letters or figures when he came to London, and entered a dissecting room under the direction of his brother. Sir Walter Scott says, "the best part of every man's education, is that which he gives himself." Neither Locke nor Bacon could be induced to stay at college long enough to obtain degrees. Are these instances significant of anything wrong in our system of education, or would the individuals noticed have been greater had they all been rigorously confined in early life to the routine of studies prescribed by the colleges?

The opinion is gaining strength daily that while a regular systematic education may be of great value to an ordinary mind, it may from the restraints that it imposes upon a good one, dwarf it. Constituted as John Hunter was he would likely have been injured by having been compelled at the time he entered the dissecting room to spend two or three years in getting a knowledge of Greek and Latin, and the same length of time in posting himself up in figures, logic and the like. Would not such a course have levelled the great naturalist down to the *status* of an ordinary man? When Hunter left his home and traveled on foot to London he was unconscious of the amount of force he possessed, or of its character with reference to any particular pursuit. His object, from all that we can learn of him, was nothing much above bread and butter. But his mind was fresh. It had not been surfeited with things uncongenial; and once engaged it traveled with delight. Every step was characterized with the discovery of a truth. What to many would have been disagreeable labor was to him enjoyment. One discovery created the desire for another, and thus he passed his useful life, accomplishing more than any other medical man.

It may be said that what is true of minds naturally great, is not true of the character of mind possessed by the larger portion of mankind—that while our educational systems might benefit the latter, they might by curbing or giving an unnatural direction to the faculties of the former prove injurious. But how are we to know

what a boy's mind is until the greater part of his education is completed?

From what we have observed of mental phenomena we confess sympathy with those who believe in a difference in the way of natural endowment. Nature has but one pattern for all who belong to the genus homo, but it is the proportion of what enters into the composition of each individual that makes the difference. Of the many who study Logic, how few are there who reason well! And of those who practice in our profession, how small the number who comprehend the extent of the science, and are able to handle properly the facts connected with a complicated disease?

With respect to the ruling idea suggested by the title of the book—"Mind and Matter," Sir Benj., so far as theology is concerned, is orthodox. While he recognises the dependence of mental phenomena upon nervous structure, he at the same time believes that the mind is capable of an independent existence. "There is nothing," says he, "unreasonable in the universal expectation of mankind, (so universal that it may be regarded as an instinct,) that there is something in us which will remain and be capable of perception and thought, and it may be of pure and high aspirations, when the gross material with which it is now associated has become resolved into its original elements."

The desire of man to live in society, says Sir Benj., is as much an instinct in him as it is in the bee, or the beaver, or prairie dog. Ought not this to settle the disputed question as to the existence of a moral sense, the innate power of distinguishing right from wrong, justice from injustice?

In regard to the two questions raised here, the separate existence of the mind and an innate moral sense, the author seems to be influenced in his conclusions more by his desires than by the facts. We simply know nothing of the mind disconnected from the body, though this by no means proves that it does not exist independently of the body. We know nothing of a moral sense, justice and injustice, right or wrong, without education. All the facts are here Still we regard it as a species of logical arrogance for any one to say that the moral faculties are created by education. About such questions we know nothing, as Priestly has well remarked.

We have the question formally raised, as to the moral and intellectual qualities of inferior animals, and the doctrine stated that in

essence these are the same as in man. The scattered observations of Lord Brougham, Cuvier, Lock, Dugald Stewart are referred to as strengthening the question of identity. Sir Benj., goes however still further. He thinks brute animals possess also the power of "abstraction" to a certain extent.

While discussing the mental principle. its qualities and modifications in inferior animals, allusion is made to its *divisibility*, and the *diplozoon paradoxon*, as an example, is cited. This parasitic animal consists in fact of two animals united in the centre so that they have a part of their viscera in common, but with two distinct nervous systems, which it is supposed would enable either half of the individual to live, though separated from the other.

We have now devoted as much space as we can afford to this work, after having read it carefully and re-read portions of it. The work is worthy of perusal from the simple fact that it in all probability shows about all of which the British mind is capable with reference to metaphysics. Sir Benj. has invested the subject with some degree of interest. On the whole, however, we do not see why he should have regarded himself as under any obligations to humanity to write this book.

Transactions of American Medical Association. Instituted May, 1847. Vol. xi. Philadelphia, 1858.

The present volume, for size, is larger than its predecessor of the year '57. It contains over one thousand pages, got up in the usual style.

We have previously published the minutes. The volume is filled up mostly with reports of Standing and Special committees. Many of these reports are elaborate, and should be in every library.

After the address of the President, Paul F. Eve, of which we have spoken previously, the volume opens with "a Report on the Medical Topography and Epidemic Diseases of Kentucky," by W. L. Sutton, M.D., Georgetown, Ky. This report occupies some 87 pages, and is written with some ability. We do not, however, wish to be understood as endorsing every thing that we see in it.

Is it expected that these reports give a history of every thing that bears in any way on the diseases of the region to which they relate, including anomalous affections, physical, mental, and moral?

or should they be confined strictly to topography and staple diseases—the epidemics that appear to be connected with the locality? We ask these questions because we see a large part of Dr. Sutton's report taken up with an account of what he terms "*the Jerks,*" or "*Epidemic Epilepsy,*" an affection resulting from long protracted religious excitement. Are such phenomena peculiar to Ky.? Do they not occur everywhere, where the human mind is goaded to excess with religious subjects. They are not even incidents, we suppose, to the christian religion, much less to its teachings in Ky.; but are a part and parcel of the workings of fanaticism in all religions.

Nor have we been of the opinion that a standing committee on Epidemics was under any obligations to hash up reports of predecessors, for the purpose of making one for itself. Dr. Sutton, has, in making out his report, given an account of *Milk Sickness*, from reports by Drs. Barbour, Seaton, Drake, etc. Dr. Drake's report has long been in the hands of the profession, and needs no repetition. The few cases given by Dr. Barbour, are better suited to the journals of the day, than to a report for a learned society; for they contain nothing but what has been, over and over again, placed before the profession.

Reports to be of service, should be confined to facts. Almost any one can make himself useful when this rule is observed. Place the facts before the profession, and its combined judgment will evolve from them, sooner or later, the proper inferences. In the report before us, we have a theory proposed, viz: that *Milk Sickness* is not a disease *sui generis*, but a form of malarial fever. In this view, both Dr. Sutton and Dr. Barbour concur. We have heard of this theory before. It has been entertained by some very clever minds. But we have always observed that those who urged it most, possessed but little experience. This is Dr. Sutton's condition; and from what Dr. Barbour has reported, we should think it also his. The testimony of a great many good observers, with large experience, is, that the disease is often complicated with malarial fever, and occasionally with typhoid.

There are some characters relating to Milk Sickness, in regard to which most all observers are agreed. These are *muscular debility, vomiting*, and *constipation*. These, indeed, have been looked upon as pathognomonic. Nevertheless, in two of the best marked cases

that we have seen, there was no constipation; but in the place of it, painful discharges of a dark pitchy character. In another case which recovered, the constipation lasted fourteen days.

What we have observed of the origin of the disease, induces the belief that it never occurs from using the milk, or eating the flesh of animals, except the animals had been grazed on wild pasture. The disease has disappeared from every neighborhood where it was formerly endemic, as soon as the lands were cleared up and put into cultivation. Again, those regions famous for malarial affections, throughout Ohio, have suffered very slightly from milk sickness. It has occurred mostly on the oak table-lands. We may just add, if the disease is malarial in character, it differs from any that we have seen or heard of. It is a new form.

The report on the Medical Topography and Epidemics of New Jersey, by L. A. Smith, occupies some twelve pages. That of Geo. Mendenhall on the Epidemics of Ohio, only about three pages. The report of Dr. Smith of New Jersey contains some suggestions in regard to the use of veratrum viride in febrile affections characterized by excessive action of the heart and arteries. The author says, "I have prescribed this remedy with pleasing effects and do not hesitate to recommend it in preference to those usually resorted to for the same general purpose." Other testimony is quoted by the author in favor of the drug under similar circumstances.

To what is the excessive action of the heart and arteries due in typhoid fever? Ought not this question to be settled before prescribing a remedy to curb it? The fashion just now is to regard this excess of action as an expression of the effort of the organism to eliminate the febrile poison. At any rate, until it can be satisfactorily shown that the excessive action is, either the disease itself or a part of it, efforts to check it by veratrum, or anything else, are not very intelligible. If typhoid fever is, like small pox, due to the presence in the body of a poison, the therapeutics should be shaped with reference mainly to that circumstance.

The report on Medical Literature is from the pen of Prof. A. B. Palmer, Michigan. It covers about forty-four pages, with an *Appendix* of twelve pages. The author has embraced within his review everything of an indigenous character that has been issued from the press for the last three years, there having been but partial reports for several years past. Of course the author has had to content himself with a discussion of the general character of period-

ical medical publications of the United States, "referring to particulars only so far as necessary to illustrate general views."

The report covers too much ground. It would have been much better, and more creditable to the author if he had attempted less. Still we take pleasure in stating that it is one of the best, if not the very best, we have had. The opinions expressed in regard to our periodical literature, and what the author styles "Original Medical Publications," indicate a good compass of information and more than an ordinary amount of judgment.

The Appendix is valuable for the information it contains in regard to "home and foreign productions," number of periodicals in the country, reports of asylums, monographs, etc. The reader will be surprised to see the large number of foreign books re-published in this country within a very short period. The same practice extends to periodicals. Notwithstanding we have thirty or more periodical journals of our own, we re-publish and circulate four of foreign origin—the *London Lancet, British and Foreign Medico-Chirurgical Review, Braithwaite's Retrospect, Ranking's Abstract.*

In concluding our notice of this report, we cannot avoid the temptation to suggest that we have an endemic in certain parts of our country for *book-making.* Teachers of medicine are very apt to think that their usefulness will not be appreciated unless they get up a book on the department they have in charge. Some, indeed, do not imagine that they have served the generation, without getting up about one on each of the departments. This mania for book-making is one of the reasons why our books do not supersede those of foreign origin. If we had fewer books, written on more capital, their success would be better. Any one can compile, but there are few who can write from what they know. Mere compilations are soon appreciated and avoided, while works that show the knowledge and labor of the author, live always.

S. M. Bemiss, Ky., contributes a report on the Influence of Marriages of Consanguinity upon offspring. This report takes up more than one hundred pages and is founded upon eight hundred and seventy-three observations made in twenty-five of the States of the Union. The observations have been arranged upon tables in classes corresponding with the estimated grades of relationship, as follows:

1. Instances of marriage, or incestuous intercourse between brother and sister, or parent and offspring.

2. Instances between uncle and niece, or between aunt and nephew.

content himself with a discussion of the general character of period-

3. Marriages between blood relations, who are themselves the descendants of blood relations.
4. Marriages between double first cousins.
5. Marriages between first cousins.
6. Marriages between second cousins.
7. Marriages between third cousins.
8. Marriages irregularly reported, all first cousins.

Our readers will recollect that we published in the Journal some time since, from the pen of Dr. Bemiss, an article on the same subject. In the report before us the author says: "I feel satisfied however that my researches give me authority to assume that over *ten per cent.* of the deaf and dumb, and over *five* per cent. of the blind, and near *fifteen* per cent. of the idiotic, are the offspring of kindred parents, or of parents themselves the descendants of blood intermarriage."

The consanguinity tables comprise 789 marriages of cousins, 246 of which have given issue to deaf and dumb, blind, idiotic, or insane children.

From data before the author, he estimates that there are now in the United States 6,321 marriages of cousins, giving birth to 3,909 deaf and dumb, blind, idiotic, and insane children.

"A very cursory examination of the tables of my report," the author remarks, "will suffice to show that *pari passu* with the increment of the same blood, the sum of defects of offspring is likewise increased."

If it is true, as suggested in this report, that a large proportion of those who are maimed intellectually and physically, owe their condition to multiplication of the same blood by in-and-in marrying, how are we to explain the increase of mankind from the original Adamic stand point? or from the Abrahamic? In the first instance in-and-in marrying was a necessity; in the other a patriarchal usage adhered to with religious pertinacity. Again, nothing appears to be more in consonance with the right expression of fact, than that races, strongly characterized, cannot amalgamate without mental and physical degradation. The offspring may, intellectually, rise above the *inferior* race, but physically they will fall below it. So that, so far even as the inferior race is concerned, it is injured by amalgamation. The superior one suffers both mentally and physically. From the facts therefore of Dr. Bemiss' report against in-and-in marrying of the same race, and those elicited on the amalgamation of different races, there seems to be a paradox. Nature, perhaps, abhors too

2. Instances between uncle and niece, or between aunt and nephew.

close a relationship on the one hand, and is dissatisfied with too distant an one on the other.

E. Andrews, of Illinois, makes a report of some length on the "*Functions of the Cerebellum.*" The report is grounded in part on the author's own researches. That the reader may get the drift of Dr. Andrew's researches, we offer a remark or two on the cerebellum. Located within the cranium, the cerebellum, from a *priori* probabilities, has a relation to sensation, voluntary motion, and the instincts. It, as has been repeatedly shown, is insensible to irritation, and may be all cut away without producing pain. If, however, the tracts (*crura*) of nervous matter which connect it with other portions be pinched, the most acute pain and suffering is produced. If sliced off, or disorganized by disease, there is more or less loss of sensibility; yet animals from which it has been removed were able to see, hear and smell. Irritation of it seems to produce no movements of any kind. Flourens and Bouillaud extirpated the cerebellum of birds by successful layers. These experiments were followed by feebleness and want of harmony of movements. The animals lost the power of flying, walking or standing, though they re'ained sight and hearing. The same experiments repeated on other animals were followed by similar results. As a consequence of these experiments it has been inferred that the cerebellum has nothing to do with general or special sensation, nor with voluntary motion; but that it is the organ for the *co-ordination* of voluntary movements —that is, it is the source of power which presides over, not the action alone of a single muscle; but the *combined* action of muscles. This theory has been repeated in all the late text books.

The paper before us of Dr. Andrews does not in the general controvert the doctrine to which we have alluded. He says, "the muscular system of mammals and birds is the part upon which the cerebellum obviously exerts most of its influence whatever that influence be." He doubts, however, its office of co-ordination. "Although it may be true," he remarks, "that through it the mind coordinates the muscular action, yet that is not true which is stated by Carpenter and others upon the subject, viz: that the size of the cerebellum is in a direct ratio with the number and variety of coordinated movements which the animal is capable of exercising.

Phrenologists have claimed the cerebellum as the organ of the sexual instinct, or amativeness. This notion has had but few, if any, facts in its favor. While against it, there have been some of a very

conclusive character. Cruveilhier and Longet give a case of complete disorganization or absence of the cerebellum, without loss of sexual passion. M. Flourens removed the cerebellum from cocks without destroying the sexual desire, though they were incapable of gratifying it.

The report on the *treatment* of cataract, by Mark Stephens of New York, presents but little, as far as we have observed, except what may be found in the books. It is, however, a very complete digest.

The author goes against a repetition of the operation of couching, which he believes unnecessary, all being gained by a single operation, if the lens has been well exposed to the action of the aqueous humor, that can be by repeating it. He answers to the question "*Is it proper to operate when one eye is sound?*" in the affirmative. This, to say the least of it, is *fancy* surgery. Taking even the best view of it, by supposing the operation successful, the focal distance of the eyes would, of course, be different, and the vision of the patient, as a consequence, imperfect in the power of definition. Dr. Stephens gives a case operated on by himself, in which, it is alleged, that this contingency did not follow. Still, who that knows any thing the crystaline lens, can believe that its removal would make no difference in the refractive power of the eye? The position, therefore, of Dr. Stephens, is simply ridiculous. He might just as well contend that one of the combinations could be removed from the object-glass of a microscope without interfering with the focal distance of the glass.

The *Report on the Medical Jurisprudense of Insanity*, by C. B. Coventry, of New York, covers about fifty pages, and is a document that seems to be pretty carefully compiled.

The first part of this report comments very liberally on the prevailing ignorance of insanity. We are told that the people know nothing about it; that lawyers and judges are equally ignorant; and that, as a consequence, "it must be admitted that in a large proportion of cases, the verdict of a court is dictated more by popular influence or prejudice, or by the talent and ingenuity of counsel, than by the stern dictates of justice."

The author notices the various definitions given of insanity, and prefers the following: "*Deranged or defective mental manifestations, arising from defective organization, imperfect development, or diseased action of the whole or part of the brain.*" Dr. Brigham

says, "Insanity, in most cases, is caused by slight disease of a small part of the brain. In most cases of protracted insanity, some traces of disease are found on a post mortem examination."

The author invokes the theory of Gall and Spurzheim, viz: that the brain is not a single organ, but a congeries of organs, to explain how it is that in many cases, only a few of the faculties of the mind are disordered. Before this theory is pressed any further, it might be well enough for its advocates to tell us the particular part of the organ devoted to memory, the part devoted to judgment, to volition, etc. How it is again, that we know nothing of the existence of either of the faculties enumerated, independently of sensation? "Every thing," says Aristotle, "that is in the mind, was previously in the senses." The old philosopher's notion has been endorsed by the most able ideologists of every age since his time.

A good while ago, the innumerable phenomena which constitute the human understanding, were regarded as but modifications of the faculty of *Perception*. *Sensibility*, by which we receive impressions from within or without; *Memory*, or the faculty of re producing impressions or sensations previously received; *Judgment*, or the faculty of perceiving the relations between sensations, *Desire* or will; were all regarded as nothing more than modifications of the faculty of Perception. Thus taught Magendie, a quarter of a century back. What has since been added to this worth recollecting? Phrenologists have proposed a large addition in the way of faculties—intellectual, moral and animal; and, as all know, have proposed certain portions of the periphery of the cerebrum as their seat. But every day's experience furnishes evidence of the inconclusive character of their reasonings. There is no secretive organ in the body but what, in one sense, has a plurality of functions, but what elaborates from the blood submitted to it, quite a number of compounds differing from each other. We say the kidneys secrete urine—that their primary office is to draw from the body its surplus water. We say, also, that it is the office of the mammary glands to secrete milk for the nourishment of the young offspring. But look at the variety of substances in urine besides the water—urea, uric acid, coloring matter, salts of soda, potash, lime, magnesia, ammonia, and a trace of silica. In milk, the number of elementary substances is still greater. Here we have a compound containing all the elements necessary to the building up of the organism. The

albuminous group of nutritious articles is represented by the caseine; the oleaginous, by the butter; the aqueous, by the water; the saccharine, by the sugar of milk. Besides, milk is rich in phosphates for building up the bones and nervous system. We might allude to all the other glands of the body, compound and simple, even to the mucous follicles, and show that they separate from the blood, not merely one simple principle, but a great number, arranged together in the form of compounds.

Why not, therefore, carry the doctrine of *speciality* of function to all the secretory and excretory glands of the body? Why not, for example, say that we have a cell in the liver especially set apart for the secretion of glycocholic acid, another for the secretion of taurocholic acid, and another for cholesterin. This would not, however, be making any progress, for the substances noticed are themselves very complex compounds.

Now, is there any thing more unreasonable in supposing the cerebrum as capable of performing different offices, all having a necessary relation to each other, than there is in supposing a gland capable of separating different substances from the blood?

The author's classification of Insanity is after that of Guy, in his work on Medical Jurisprudence. 1st. *Amentia*, including Idiocy, Cretinism and Imbecillity; 2d, *Dementia*, consequent on mania, mental shocks or injuries of the brain, (*a*) senile; 3d, *Mania*, (*a*) general, (*b*) intellectual, (*c*) moral. This, it will be observed, leaves out cases of insanity resulting from onanism, which do not seem properly to come under any of the above divisions.

The subject of *Moral Insanity* is broached in this report. We see nothing, however, that throws any additional light on the question, "how far this form of insanity should excuse the commission of crime?" Most of those who are acquainted with the subject think, that as long as the individual is able to control his actions, and possesses sufficient intelligence to know the consequences of his actions, and is not laboring under any delusion, it is difficult to say how far he should be excused. Several forms of moral mania are given: *Cleptomania*, or propensity to theft and lying; *Pyromania*, or propensity to incendiarism; *Suicidal* Mania; *Homicidal* Mania.

Tests of Insanity.—The report posts up the tests that may be relied on to determine the existence of insanity. We select those of most value:

"1st. Every thing should be learned in regard to the previous history of the individual; and as of prime importance, the education he had received.

"2d. The standard by which an individual is to be judged *is his former self.*

"3d. The maniac differs from the sane, not from a loss or defect of any of the faculties, as in idiocy and imbecility, but in excessive action in some, and in exercising them differently. * * It is not mere eccentricity that constitutes insanity, or that a man feels, thinks and acts differently from other men. The senses are often changed, sometimes preternaturally exalted.

"4th. The insane antics of mad men are usually the result of their delusion.

"5th. The acts of mad men, which are the result of delusion, are often such as no sane man would deem proper to accomplish the object in view.

"6th. The violence of the insane is often the effect of his delusion, and not of mere passion.

"7th. Insane patients of a reserved disposition, unless impelled by some powerful motive, have often the power of concealing their delusion.

"8th. The acts of an insane man often evince the same forethought and preparation as those of the sane.

"9th. The insane, notwithstanding their proverbial cunning, are easily imposed upon.

"10th. Insane persons are often conscious of their condition, and understand the legal relations in which they are placed."

The author discusses briefly the question, "*How intoxication affects responsibility for crime?*" He reiterates the doctrine very current now, that insanity, when produced by intemperance assuming the form of delirium tremens, affects responsibility in the same way as insanity produced by other causes. The argument that because it arose from *voluntary* intemperance, is regarded as having no force, as a large proportion of the cases of insanity from other causes arise from vices or follies voluntarily committed. Insanity, immediately produced by intoxication, does not destroy responsibility, except in cases where, from previous injury of the brain, or from disease, or peculiarity of constitution, intoxication ensues from a quantity of liquor which would not affect a person differently constituted.

We have thus passed over the principal parts of this report. And we may say in conclusion that the author has presented a very good summary of what is regarded as of most interest on the subject.

In a conversation, not long since with a gentleman of long experience in the management of the insane, he remarked: "I have often been struck with how *little* they (the insane) know; and with how *much* they know. When talked to on matters connected with their delusion they seem to know nothing, but on all other matters they are intelligent." This gentleman remarked that while having charge of one of the asylums of our State, he not unfrequently found that when he needed information on minor matters with which the lunatics, previous to their derangement had been acquainted, he could consult them with advantage, provided the subject involved nothing connected with their own form of trouble. For example, if a horse sickened or became unruly, it was only necessary to hunt up among the patients a man whose avocations had made him acquainted with horses and their diseases; and the information desired was obtained just as well from him as if he had been perfectly sane. So in regard to legal questions. A patient that was in the asylum could give with great accuracy all the law bearing in any way on the case at issue, and tell beforehand what, in all probability, would be the decision of the courts.

The present age has nothing of which it may with more propriety be proud, than its knowledge of insanity. History fails to inform us of the existence of any people who have had any proper conceptions of the subject. Not being comprehended, of course no adequate provision was made for the treatment of the disease. Previous to the time even of Pinel, insanity was considered almost, if not entirely, an affection of the intellect, or of the reasoning faculties. Fricks and lesions of the affective faculties were scarcely at all recognized.

If, therefore, the present generation should be known to posterity only by its architectural monuments for the care and treatment of the insane, as the ancient Edomites are known to us only by the ruins of Khashne, their theaters, and catecombs at Petra, we would have no cause to complain of the opinion that might be formed of us. An index would be furnished by which the character of our civilization would be fully appreciated.

We must here close our notice of this volume. To quite a number of papers, including the "*Prize Essays*," we are unable now to pay any attention.

The Science and Art of Surgery, a treatise on surgical injuries, diseases and operations. By JOHN ERICHSEN. An improved American edition, from the second enlarged and carefully revised London edition, illustrated by four hundred and seventeen engravings on wood: Blanchard & Lea.

The first edition of this work is now extensively in the hands of the profession. This fact supercedes the necessity, if it does not indicate the impropriety, of an extended notice. It appeared about four years since and is already exhausted. We now discharge a pleasing duty in announcing a second edition, materially enlarged, and thoroughly representing late improvements.

After careful and frequent perusals of Erichsen's surgery, we are at a loss, fully to express our admiration of it. The author's style is eminently didactic, and characterized by a most admirable directness, clearness and compactness. These traits have enabled him, in a volume of about 1000 pages, largely occupied by wood cuts, to present what is, in many respects, the most full and complete systematic treatise, on the subject of which it treats, in the English language.

As illustrative of these features of the work, we name what is, perhaps, the most vulnerable chapter in it, that on inflammation. The author disposes of this intricate topic in forty pages. Yet by avoiding the polemic aspects of the subject, a careful and discriminating rejection of what is unsettled and uncertain, and an admirable exercise of what we regard as his peculiar talent, he has succeeded in embodying, in this brief space, almost everything important, that is known on the subject. The doctrines of the chapter are, furthermore, thoroughly modern, having received a very decided and characteristic impress from the extensive and able observations of Paget.

The present edition contains about 100 pages of new matter. The discussion of many topics is so altered as to be essentially new. In this connection we might mention the discussion on the subject of morbus coxarius, and that on flexion of the knee joint. Among the improvements inserted we notice, of American origin, Reed's method of reducing the femur by manipulation, and Crosby's mode of reducing dislocations of the proximal phalanx of the thumb. Cassaignac's ecraseur, in its application to the extirpation of tumors and excision of the tongue, is presented with illustrations.

In the discussion of the subject of foreign bodies in the air passages and ovariotomy, the book of Professor Gross, and the essay of Dr. Lyman of New York are brought prominently into requisition. In fact, a catholic spirit, the most ample literary resources, and great breadth and scope of view, as evinced by a thorough familiarity with, frequent allusions to, and impartial use of American, French, German, Russian and other authorities, characterize the work throughout.

A feature calculated to commend the work to the profession is the abundance of its original illustrations. Though as specimens of art, they are not of a high order, they have, with little exception, the merit of originality, and delineate very fairly.

We commend the work to the profession, as one of rare ability and excellence, and that no practitioner, burdened with the responsibilities of surgical practice, can afford to dispense with.

As to the execution, perhaps the highest compliment which it could receive, is, that it is in the best style of the house from which it emanates. H.

A Treatise on Fractures. By J. F. MALGAIGNE. With one hundred and six illustrations. Translated from the French, with notes and additions, by JOHN H. PACKARD, M.D. Published by Lippincott & Co..

But few books, laying claim to completeness, have been written on fractures as an exclusive subject, either in ancient or modern times. Yet it is not to be supposed that as common and serious injuries as these failed to elicit a full share of attention, at the hands of surgical writers. A long list of the most eminent names in the history of the profession might be adduced, as intimately associated with this department of surgery. Yet their productions were, for the most part, fragmentary. Among our Gallic brethren, although Desault, Verdue, Duypuytren and others have written largely on the subject, we believe the work before us is the first that claims to be a complete and exclusive embodiment of what is known on the subject. In this particular they are in advance of their British neighbors. Although the latter have produced a number of special treatises on the subject, such as that of Amesbury, Lonsdale, Stanley and R. W. Smith, none of them are full and complete as is the work under consideration.

The book before us is a well executed octavo of seven hundred pages. The first two hundred and eighty-seven pages are occupied with the general subject, under the separate heads etiology, varieties of fracture, general semiology, course and terminations, diagnosis, prognosis and treatment. The body of the work is occupied with the very full and detailed discussion of the ordinarily recognized special fractures.

The varieties recognized and described are as follows: First, the *incomplete*, subdivided into the fissured, incomplete properly so called (consisting of bending with incomplete fracture in the long and depression in the flat bones,) splintered and perforated.

Complete simple fractures are divided as follows: Transverse, serrated, oblique and epiphyseal. Under the head of *Multiple* fractures are designated fractures with splinters, fractures with several fragments, fractures by crushing and fractures involving several bones at once; and finally, *complicated* or *compound* fractures constitute a fourth general division.

This classification, as the standard of diagnosis in actual cases, is to some extent utopian. But for the purposes of the author, it is at least convenient, in as much as under these several heads he is afforded the occasion for presenting anything peculiar characterizing these several varieties. In connection with the discussion of incomplete fractures, properly so called, he raises the question as to the possibility of uncomplicated bending in long, and depression in flat bones, &c. Again, under the head of the epiphyseal, we have a rather full history of observations on the subject of disastasis, the mode of its production, the age liable, and the question of distinctive symptoms, mode of union and treatment, the result of which seems to be, in the author's mind, a virtual rejection of the variety in actual practice, on account of the absence of cognizable distinctive features. Under the head of course and terminations, we have a description and history of the more prominent kinds of appliance, and very full discussions of various practical questions, such as the proper time for applying and removing apparatus, the movements which may be allowed the patient, theory of anchylosis following fracture, pseudarthrosis after fracture, formation and transformation of callus, hindrances to consolidation, &c. These we regard as among the most interesting and important discussions in the book.

Under the head of non-union, the author adduces the views of M. J. Cloquet, in regard to a source of delay in connection with the

immovable or starch apparatus, which we do not recollect to have noticed in any other treatise, but which had previously been impressed upon us, by observation in our practice, as perhaps the most serious objection to that mode of treatment. Probably most practitioners, who have treated a dozen broken legs after the plan of Seutin, have observed that, in the latter part of the treatment, the limb becomes remarkably soft and flabby; that its temperature is impaired, its circulation and nutrition imperfect, the part tending to atrophy with more or less edema, assuming a white waxen appearance, the epidermis presenting ptyriasis, or in well marked cases phlyctenula. This condition Cloquet attributes to the conjoined effects of non-use, pressure, and exclusion from light and air, mainly the latter. He regards it as a fruitful source of non-union, and designates it as local cal scurvy. It has been impressed upon us that the grade of action in parts thus situated, was such as not to allow of a vigorous reparative process. This would seem to require an unrestricted circulation and nutrition, and a high grade of vitality about the seat of the fracture. This incident is worthy of careful observation.

The plates are, for the most part, illustrative of specimens which the author has collected from various sources, and equally so of the imperfections characterizing treatment. The translator remarks in his preface, that "for greater facility of reference, they have been collected at the end of the volume, instead of being interspersed throughout the text." They are fine specimens of wood engraving, on superior Bristol board.

The book presents a liberal amount of original observation and is indicative of great industry, honesty and erudition, on the part of its author, and abounds in interesting and important historical details. Without, however, attempting to assign the proportion of responsibility which belongs respectively to the author and the translator, we feel compelled to say that the style of the book is not characterized, in an eminent degree, by directness and perspicuity. This has been impressed upon us as perhaps the only feature tending to mar its reputation as a great work. In every other particular it is a model of scientific literature. Notwithstanding this, and the fact that the original publication was made only about twelve years since, by virtue of it the author's influence, in the surgical world, is already, in a very important sense, ubiquitous. The constant allusions to it in text-books, as an authoritative work,

are such as to constitute the highest compliment which any work can receive.

No practitioner who is responsible for a large amount of practice, in its department, can afford to do without it.

H.

A Manual of the Practice of Medicine. By T. H. TANNER, M.D. F. L. S. Author of a Manual of Clinical Medicine, and Physical Diagnosis, etc., etc.; Licentiate of the Royal College of Physicians; late Physician to the Hospital for Women. First American, from the Third Revised London Edition. Philadelphia: Lindsay & Blakeston, 1858. (For sale by Randal & Aston.)

There seems to be a rage just now for *Manuals*. Each department of medicine has its Manual. Why is this? Can't as much time be afforded now to look through elaborate treatises as formerly? Or are these useless? Is it unnecessary just now to buy a book that holds its subjects before the mind until they are entirely exhausted?

This is a fast age; and besides, there is no doubt about our having much in medical literature that is worse than redundant. Still, it is a question whether what we have can be, without damage, compressed into less than one-fourth of its former dimensions. Take Practice, for example, of which this book treats, and is one-half of what we have on this department by Drake or Watson, useless? We think not. If a writer attempts to give a portrait of a disease, he should not content himself merely with the outlines, because these are liable to misguide; but should paint the picture so complete, that the original may be easily recognised.

Hand-books are thought to be applicable to the student. Perhaps they are in some respects. But may they not prove injurious by inculcating short-cuts, and destroying the taste for thorough, elaborate investigation. Bad practices, as some one has said, are harder to *un*learn than to learn.

The little volume before us is a model in its way; terse, and as far as possible, comprehensive. It is also up to the very latest researches. But there is not enough of it for practical purposes. It is unnecessary to those who are well read; for they have it all

and much more besides; while to the student, it is not sufficient. More than all, the dogmatisms, inseparable to such condensation, are far from being palatable.

The work commences with a chapter on *Inflammation.* This is disposed of in some seven pages. We have looked through this chapter, supposing it to be a fair specimen of the author's performance. We see nothing new on the *causes* or *terminations;* but there is something on the treatment which, though not new, deserves a passing notice. The *antiphlogistic regimen,* so long in vogue, is called in question, as having always been wrong, or as being not adapted to the types of modern diseases. Our author, with Prof. Bennett, adopts the former hypothesis, and says, that inflammation cannot be cut short, and that our knowledge of physiology, pathology and therapeutics, agree in suggesting to simply *guide* the morbid process to a favorable termination, just in the same way as we, at present, try to conduct cases of typhus, small-pox, scarlatina, through their natural progress, without making heroic efforts to cut them short. In treating inflammation, continues the author, the object should be to support the vital powers instead of lowering them, and it should further be to promote the excretion of effete products.

In support of this plan of treatment, the author cites its success in the hands of Dr. Bennett, physician to the Royal Edinburgh Infirmary. Dr. B. treated 65 cases of pneumonia, of which number 62 were dismissed cured, and three died; that is, one in 21⅔. These cases, so far as character of disease and complication were concerned, were supposed to present a fair average.

This shows a success much greater than that of Louis, who treated 107 cases of the same disease by bleeding and tartar emetic, with a loss of 32; or than that of Rasori, in the Hospital of Milan, who treated 648 cases by large doses of antimony, with the loss of 143.

But is such showing conclusive in a matter not only important, but where there is room for a great variety of circumstances calculated to effect results. Is there no fallacy here to start with in the number of cases treated by Dr. Bennett? Should our views be revised on evidence derived solely from 65 cases? We had supposed that conclusions touching the value of any plan of treating disease required a much greater number of cases to be entitled to confidence. Again, do the cases of Dr. Bennett present every thing in the way of age, sex, habit of body, endemic influences, portions of pulmonary apparatus affected, extent of disease, and complications? The pneu-

monia of old persons is a very different disease from what it is in children, or even in adults. In the aged the inflammatory process diffuses itself over the pulmonary surfaces with facility, and hence the great mortality in this class. In children the disease is apt to be lobular, and the prognosis, as a consequence, favorable. In the former the treatment must be supporting from the commencement; in the latter, where the the evidences of repletion are obvious, it should, we had supposed, be antiphlogistic.

We have thus noticed the disease as connected with the extremes of age; and we are certain that we are not transcending the limits of truth, or the most trustworthy observations when we say that the treatment not only differs, but is exactly opposite.

We might, if we had space, go on and point out how other circumstances call for a modification in treatment. An adult of full habit and presenting evidence of great engorgement of the lungs, the disease being primary, would be benefitted by remedies calculated to lower the action of the heart and arteries; whereas the same individual, if emaciated and suffering with the disease as a complication, or as a mere extension of morbid action from some other part, would not only not be benefitted by the plan just mentioned, but would most likely be injured. Again, the pneumonia of malarious districts requires a special system of therapeutics. Here it may be remarked, that it is not those articles or measures that impart tone or take it away, that cures the disease, but quinia, of the modus operandi of which we know nothing. In short, what we find true of pneumonia confirms the medical maxim, "*No two cases are alike.*"

We confess, therefore, that we are unprepared to adopt the views of Bennett in regard to inflammatory diseases without other evidence of their correctness than what is furnished by the results of 65 cases of pneumonia, or his statement, that those practitioners having the greatest amount of experience, combined with the greatest knowledge of physiology, pathology and therapeutics, are now agreed that the antiphlogistic plan is wrong.

It will be noticed by the reader that the evidence we have been noticing is of the empyrical kind, the back-bone of all medical evidence, but at the same time liable to an almost infinite variety of fallacies. That empyrical knowledge be worth any thing, the observations should be long continued under every possible variety of circumstances; they should be made by eyes, ears, that have been carefully disciplined, that have been trained by long use, with a practiced judgment.

Pathology we should suppose ought to have something to do with the treatment of disease—at any rate this was formerly so axiomatic, that every one writing on the subject took structural changes very fully into consideration. We do not understand Prof. Bennett to ignore pathological considerations in the treatment he proposes for acute inflammation; but what he does is the same in effect. He denies the generally received theory of the process and substitutes one in its place that he himself has invented. This circumstance, if no other, in our judgment, ought to have the effect of placing his notions of treatment at a discount.

We have had the medical mind for twenty centuries, or more, at work on the subject of *inflammation* and so far as treatment is concerned, the general tendency has been during all this time to look upon the views of Hippocrates with favor. His great mind seemed to grasp the leading features of the disease; and he is bold in the expressions of his opinion of the value of the anti-phlogistic plan in the proper cases of pneumonia.

We have no great deal of veneration for much that is found in the ancient records of medicine. But when we see a plan of treatment that has lasted for centuries, and that has withstood the shocks of innovation under a great variety of circumstances; and that, moreover, seems to be pointed out by the most enlightened views of the nature of the structural changes, we confess an unwillingness to discard it, except upon the most conclusive evidence, that there s something better.

Of Nature and Art in the Cure of Disease. By Sir JOHN FORBES. S. S. & WM., 389, Broadway, New York. 1858.

Brief Expositions of Rational Medicine, to which is prefixed the Paradise of Doctors. A Fable. By JACOB BIGELOW, M. D., late President Massachusetts Medical Society. Boston: PHILLIPS, SAMPSON & Co., 13 Winter Street. 1858.

The first of these works, Nature and Art in the Cure of Disease, was noticed some time since in our Journal; the other one, by Dr. Bigelow, of Boston, has been on our table for some months. The scope of both is the same. Both are down on what they are pleased to term the *heroic method* of treating disease. They also agree in leaning very strongly to the "expectant method."

It is very probable, that here, in the United States, where dis-

eases are, in many localities, very bold, and, from the commencement, dangerous, we err sometimes in doing too much. But, is an error here any worse than one in the opposite extreme? A careful "expectant" plan in many diseases, particularly those of a chronic character, enables a physician to study the natural history of the complaint, enables him more clearly also to see what assistance he may expect from nature. This plan, therefore, has always been adopted by those who have understood themselves. It is just what common sense could not fail to suggest. We have, however, a class of diseases of a very different character; diseases that sometimes jugulate nature, if not interrupted, in a very few hours. We allude, among others, to our "congestive intermittents." What is to be done with such? Must the practitioner stand over a case of this kind, studying the natural history of the disease, and looking to see what nature is able of herself, to accomplish? or "pitch in" and rescue the patient from danger consequent on a second paroxysm?

The works before us have been written with some capital in the way of experience, as any one can see who peruses them; but not with all the experience necessary. We suppose there are fashions in medicine, as in many other things; but it is very questionable whether these fashions are always purely artificial. Medical men have been found, in certain periods past, relying very much on purgatives, blood-letting, mercurials, emetics, and the like. But who is prepared to say that such practice, under the circumstances, was wrong? Who is prepared to say that it was not the best that could have been instituted? Was it because of its injurious effects, that a particular mode of treatment was abandoned, or because of a change in the character of diseases? The mild plan, recommended by the works before us, was tried for a while during the early part of the present century, at the suggestion of STAHL, but was, by general consent, abandoned as "*a meditation on death.*"

We by no means wish to be understood as endorsing the heroic practice in all cases, even of acute disease. Some of these are, as all know, amenable to bold, vigorous treatment, when well timed; others are not, because of a cycle of changes through which they will run irrespective of all known remedies. Then we have the complaints known as chronic, to which a medication is applicable that s neither heroic nor expectant, but one that restores to the natural state. The quantity of medicine used, may be much or little, but it must be sufficient for the purpose. Nor do we wish to be undertood as ignoring the curative powers of Nature. Every one has

confidence in the *vis medicatrix naturæ*, and knows that it is equal to a certain class of emergencies, but it is because it is not always equal, that the necessity for artificial interference has been recognized by both civilized and barbarous nations, and by the Creator, who has created our most heroic remedies ready for use.

The negative plan of treating diseases, and to which the two works before us lean, as previously intimated, is not new. It may be, for aught that is known, better adapted to what happens in our day, than it was when Stahl tried it. Still, we have no idea that it will be productive of any benefit, except that it may have a tendency to correct over-dosing where it exists.

It will be noticed, by those who have read the works before us, that there is a leading sophism that pervades almost every page of both of them. They object to positives, efficient medication, because many of the more powerful articles of the materia medica have been abused. Should we leave off the practice of eating meat, because some make gluttons of themselves? Such a course would be just as reasonable as to leave off, during sickness, the use of what the Creator has provided in the way of remedies. An individual, to receive proper nourishment, requires daily so many ounces of food and water, and the quantity is positive. So an individual, the subject of disease, requires medicine in such quantities as will destroy the disease, and allow the normal actions to resume their sway.

Age is valuable, but it has its drawbacks. It is the transition stage between manhood and second childhood; and while it protects against the errors of early life, it is often found to predispose to those that surround the individual in his declining years. In their old days, Drs. Forbes and Bigler, in our judgment, have been to some extent victimized with the nonsense of Homœopathy.

PART FOURTH.

EDITORIAL.

Discussion in the New York Academy of Medicine. Dr. Green's use of the Probang in the Whitney Case.

In December last (14th,) a patient by the name of Whitney, complaining, amongst other things, of an affection of the throat, called at the office of Dr. Green (who has made himself extensively known to the reading public by his use of the probang,) and had his throat medicated. At the time, and while the patient was at the office, no complaint was made. After he went home he sent for his family physician, Dr. Beales, and complained to him that Dr. Green had injured him seriously. Soon after Dr. Beales saw him, emphysema over the chest and neck made its appearance and the patient died on the 7th day after the use of the instrument. The patient being the heir an to estate worth five million of dollars, his friends after his death and a portion of the community, charged Dr. Green with having killed him. A post mortem was made by Drs. Beales and Mott, (Prof. Valentine Mott.) This was drawn up by Dr. Mott, and with the history of the patient's illness, submitted to the Academy of Medicine where the affair underwent an investigation. After all the facts had been elicited thoroughly the academy acquitted Dr. Green of any blame whatever.

We have had no prepossessions in favor of Dr. Green in the use he has made of the probang, and have so expressed ourselves in the previous numbers of the Journal. We will do him the justice, however, to say that he has called attention to the practicability and safety of topical applications to the larynx and trachea and for this he deserves credit. Like all experimenters he has become without any doubt too enthusiastic, and has used his probang for diseases and forms of disease to which it was in no reasonable sense adapted. In, however, the case before us he was we think improperly blamed, and it pains us to relate the fact that the vulgar prejudices against him were at the outset countenanced by Drs. Beales and Mott.

The post mortem revealed an opening near the bifurcation of the bronchi, a cavity in the upper part of the left lung, and a hole leading from this cavity that extended through the pleura pulmonalis and pleura costalis; and present also was inflammation and hepatisation of part of the left lung. Besides these lesions, *an abscess*

containing pus was found in the deep cervical fascia between *the larynx and pharynx* and communicating with the latter by an orifice large enough to admit *the finger*. This abscess was about the size of a large hen's egg. and during the attendance of Drs. Beales and Mott for a period of seven days on the patient was not noticed by either of them.

We have said that Drs. Beales and Mott favored the idea of the people, that Whitney's death was due to Dr. Green's use of the probang. This, if we had no other evidence of the fact, is very obvious from the bearing of these gentlemen before the Academy where the matter was discussed. They both exhibited much more feeling in their verbal statements than is required in the communication of facts. Besides, their account of the post mortem is, to say the least, pedantic. They stand convicted, nevertheless, by their written statement of the history of the symptoms and the post mortem of having failed, during the life of the patient, to recognize the presence of the *pharyngeal abscess*.

We have no intention, from the data before us, of expressing an opinion on the cause of Whitney's death. With such lesions as were present, either of which was adequate to such an event, we regard it as recklessness for any one to attempt to discriminate. The opening in the left bronchus, communicating with a cavity in the lung as large as a small walnut, and again with an orifice in the pleura pulmonalis and pleura costalis which gave rise to the emphysema—may have caused the patient's death. But were these lesions due to the use of the probang, armed as it was with sponge and used with ordinary care? A physician who was present at the operation stated before the Academy that the operation was not followed at the time it was performed with any uncommon complaint from the patient. Could the left bronchus have been perforated, the instrument extending into the lungs, thence through the two pleural membranes, without having, at the time, produced evidence thereof in the way of pain and other unmistakable symptoms?

The patient had *an abscess* as before suggested, *on his neck, as large as "a large hen's egg,"* and communicating, by an opening sufficient to admit the finger, with the pharynx. What caused this? When did this pharynageal abscess commence? What relation had it to the cavity in the lung? What to the general health of the individual? Was it sufficient of itself to have caused the death of the patient? Such questions should have been answered, it appears

to us, before coming to any conclusions, as it respected the cause of Mr. Whitney's death.

The progress of this case has developed certain traits of human nature in quarters not, by us, previously suspected. Persons not acquainted with the many contingences of medicine are often to be excused for crude notions, and hastiness in forming opinions. They get up prejudices against physicians, that are annoying, and it seems no amount of professional service or reputation secures exemption. Prof. Gibson's case is in point. He used an exploring needle while in Rome to ascertain the state of things in the orbit of the eye of the man Crawford. Unfavorable symptoms followed the operation; and the relatives and friends of the patient accused Prof. Gibson in such a manner, that, although he possessed an enviable reputation as a surgeon for nearly half a century in the oldest and most renowned school of America, he was compelled in self-defence to have his practice in this case endorsed by European surgeons. Nothing, we repeat, is more common than for the people to find fault with medical men and hatch up against them all kinds of charges. Such things, fortunately however, soon die off, unless medical men can be found ready to chime in and give them currency. It seems that such is too often the case. The venerable Dr. Mott with all his years it seems was not above this weakness. "*Humanum est errare.*"

Recent Improvements in Practical Surgery. Excision of the Tongue.

Mr. Syme of Edinburgh recently performed this operation in two cases after the plan proposed, and practiced in a single case, by M. Sedillot. An incision was made through the lip, and over the chin, to the body of the os-hyoides. With saw and pliers the jaw was then divided at the symphysis. The two halves were then held aside, thus giving ample room for the dissection of the parts back to the os-hyoides near which the tongue was separated, the lingual arteries having previously been tied near the cornua. Both the patients died of inflammation of the lungs after the lapse of about a week.

Mr. Syme, with commendable boldness, announces these results to the profession, holding up these cases as a beacon, and expressing the opinion that "there should be no hesitation in deciding against the repetition of the procedure."

A Paris correspondent of the Times and Gazette, announces the successful extirpation of this part, on a different plan, by Ricord. It was effected, without dividing the symphysis, by the use of the écraseur. This case seemed to progress favorably for a length of time, but cicatrization progressing, the patient lost his regained articulation, and dysphagia became extreme, threatening inanition, when the patient died suddenly of latent pneumonia.

On the other hand the same correspondent states that patients whose tongues have been extirpated, have been exhibited before the Academy of Medicine, in numbers sufficient to guaranty that the operation is not, in itself, necessarily fatal. Maissonneuve performs it very much as in Syme's cases, only that the organ is separated by the écraseur.

Treatment of Aneurism by Digital Compression. In the January number of the N. A. Med. Chirurg. Review, Dr. D. W. Gross gives what purports to be a report, in brief, of all the cases that have been thus treated. The important facts, including the results, are summed up in the following table:—

Variety.	No. of Cases.	Cure.	Failure.
Popliteal	15	10	5
Femoral	4	3	1
Inguinal	2	..	2
Arterio-Venous	2	2	..
	23	15	8

From the data, found in these cases, the writer deduces the following propositions:—

"I. Digital compression, uncombined with apparatus, was first attended with success in the hands of Dr. Knight; but to M. Vanzetta is due the merit of having first introduced it into practice.

"II. It has never been followed by bad consequences, and when not successful, it so modifies the tumor and the collateral circulation as to render a cure by other means almost certain.

"III. It has been employed alone, either previous or subsequent to mechanical compression, in fourteen instances, eight being failures.

"IV. In only seven cases has it been employed primarily and alone, and in all but two with perfect success.

"V. When double and alternating, it has effected cures in every case, five in number, and therefore deserves special attention.

"VI. In most of the cases the compression has been total; but this is not necessary for a favorable result.

"VII. It has effected cures, whether it was continued, interrupted, or intermittent; in some cases the patient applying the pressure.

"VIII. When properly employed and continued for a sufficient length of time, and the cases are suitable ones, it can scarcely fail to accomplish a cure. Inguinal aneurisms are not fit cases for this procedure.

"IX. It is less apt to give rise to inflammation of the integument, and has been borne when mechanical pressure has produced an eschar.

"X. It can be used when apparatus has failed or is intolerable. In a majority of these cases, cures have been accomplished.

"XI. In certain situations it can be made to bear upon the artery alone. It is far less painful and requires a much shorter time for a cure than any other method of treatment."

Mammary Jacket. The December number of the Atlanta Journal contains an article from the Nashville Medical Record, descriptive of an ingenious appliance, under this name, for the treatment of mammary abscess. It is a paper read before the Montgomery, Tennessee, Medical Society, in 1851, by Dr. D. C. Higbee. It is badly printed, but we think the following extract too valuable to be lost:—

"Take a strip of strong cotton, woollen, or other goods, from ten to fourteen inches wide, and sufficiently long to allow the ends to overlap each other by several inches after encircling the chest. To this are sewed two broad and short shoulder-straps, having places scalloped out of the broad piece so that undue pressure will not be made in the axillæ. Neither splints nor starch need be used to keep the jacket from rolling upward; the configuration of the chest will always keep it smooth: just the reverse will be obtained in the abdominal bandage, in parturition. This jacket is to be put on like a vest, (but next the skin,) except when the shoulder straps are secured in front by a strong pin, which is generally preferable, that we may change the position of the mammæ with ease, or that the patient may be saved the trouble of undressing when a clean one is desired.

"Now suppose the abscess already well opened. If you wish to apply a poultice, let it be smoothly spread, circular, and *thicker at the base of the gland*, or otherwise, as the peculiar shape of the breast may require. When it is made thickest at its edge the pressure on the gland will be most equalized, for the circumscribed force, acting

laterally on its base, will prevent its becoming more flattened than natural, which I consider necessary to avoid when the gland is involved. Let us never lose sight of the anatomy of this organ.

"When I am not using the poultice, I surround the breast with wool or cotton to meet the same indication. But what I should greatly prefer in many cases would be a thin caoutchouc bottle of globular shape, or a very large bladder, so that when partially filled with some liquid, or air, it might be semi-collapsed, and laid over the mammæ, and bandage over it, which would insure a uniform pressure on all parts. If there be but one breast diseased, cut a sharp oval hole lengthwise with the bandage, or jacket, through which the healthy breast may protrude, having the aperture just large enough, so that there will not be undue pressure on the gland; thus the patient may give suck without inconvenience.

"Now let the lower edges of the jacket be drawn firmly around the body and secured by pound pins. Then place your hand under the mammæ and poultice, and elevate them rather above the point they would occupy if the patient were in a recumbent posture. If the cavity be very great, it may be necessary to draw the breast laterally, to place the sinus or lips in a position favorable for granulation, or for the complete escape of purulent matter, or for both. Continue the pinning of the jacket from *below upward*, until its ends are well secured.

"My rule for tightening this jacket is this: let it be as tight as the patient can comfortably bear it. If the inflammation be very acute, and the tumefaction great, a slight pressure only may be tolerated at first, but she will soon be able to bear it tighter, as congestion and inflammation subside.

"In nearly every case this jacket should be adjusted when the patient is recumbent, for the gravity of the ponderous breast will thus be conveniently overcome. This being done, cut an opening, if necessary, through which the nipple may protrude, having already secured one through the poultice, that the milk may be drawn with a breast-pipe or nipple-glass, by a nurse, or even the child, should the lacteal ducts not be diseased. The dressing should always be renewed frequently, and that, too, *when the patient is recumbent.*

"By this mode of treatment I have never failed in performing a cure, in an ordinary case, in from five to fourteen days, and the worst case I have seen required no more than three weeks."

H.

Hints to Craniographers.

We have had this circular, by J. Aitken Meigs, M.D., of Philadelphia, for some time on our table. It is designed to show the "importance of establishing some uniform system by which the collection and promulgation of Craniological statistics, and the exchange of duplicate crania may be promoted."

The object of Dr. Meigs is to collect data on the subject of Ethnology; and the plan he proposes is this: every individual, society, or college, having made collections of skulls, should, at once, let the fact be known, either through the medium of journals, or by publishing catalogues.

It is not long since every thing pertaining to the human organism was looked upon as unique; and, as a consequence, presided over by metaphysics. Barboncois, Ray, Brisson, have followed Aristotle in excluding man from their classifications of the animal kingdom. To Camper, Blumenbach, Zimmerman, Sæmmering, we are mostly indebted for the suggestion that the scope of zoology rightly includes man with other animals; and that by the application of science, instead of metaphysical speculation, much that relates to him morally and politically, may be successfully investigated.

As Comte has well remarked, every science has, as the first part of its career, a metaphysical or speculative stage. This has been eminently true with reference to the young science of Ethnology. The few truths which have been collected together, have been made the basis of no small amount of sophistical reasoning. Men have made their appearance who have been impatient with the research and delay necessary and incident to scientific pursuits; and they have brought reproach upon the inquiry. What is most needed at the present time, is the collection and classification of facts. A few zealous laborers, both in this country and Europe, have operated without the success that would have attended united efforts; still, enough has been elicited to induce the belief that careful and patient labor, in the right direction, will give a very valuable clue to race peculiarities that may be turned to practical account, morally and politically. It is a subject but little understood by either ourselves or our predecessors.

No part of the human organism is so distinctive of race, as the skull. In it are situated the brain, the senses—the organs that es-

tablish the relations between the individual and the things around him; the organs that determine and shape the characters of mankind.

We have, perhaps, at present, in the Academy of Natural Sciences, in Philadelphia, the largest collection of human crania in the world. This collection is due to the labor of the late Dr. S. G. Morton. It numbers now about 1,100 crania, representing 170 different tribes.

Dr. Meigs has given evidence of commendable zeal on the subject of Craniography, and we hope his "hints" may receive attention.

Books, Monographs, etc., received.

Lectures on Diseases of Women, By Charles West, M.D., 1858. (Blanchard & Lee.) Part II.

Paine's Institute of Medicine, 1858. New York. (Harper & Brothers.)

Morris on Scarlet Fever, 1858. Philadelphia. (Lindsay & Blackeston)

Contributions to Operative Surgery and Surgical Pathology, 1858. By J. M. Carnochan. New York. (Lindsay & Blakiston.)

An Examination of the Question of Anesthesia. (By the Hon. Truman Smith.)

An Essay on Inflammation, By J. H. Watters, M.D. St. Louis, 1858.

The Treatment of Paralysis of Motion. By Charles F. Taylor, A. M. M.D.

Introductory Lecture, By C. B. Lindsay. Nashville, Tenn.

Typhus Fever in Great Britain, By J. B. Upham, M.D.

Physicians' Visiting List, for 1859. This is a very valuable companion for the physician. Besides the usual contents, we have Marshall Hall's *Ready Method in Asphyxia*, Poisons and Antidotes, a table for calculating the period of Utero-Gestation, and an Almanac. (For sale by Randall & Aston.)

Summer Course Lectures.—We call attention to the advertisement of our Summer Course of Lectures. Students will find it very much to their advantage to attend this course, where time will be afforded to dwell upon many important subjects that have to be passed over too hastily during the regular one.

Fee for the whole course, $30.

Those who attend the Summer Course and pay the above fee, will be admitted to the next succeeding winter session at one-half the regular price of the tickets.

Students whose ambition it is to become *thoroughly* versed in all the departments, will find it much to their advantage to attend a *Summer Course* of Lectures.

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For further information, address

JOHN DAWSON, M.D., Dean.

Commencement of Starling Medical College.—The Commencement exercises of this Institution takes place this evening March 1st, at the Second Presbyterian Church. The *Annual Address* will be delivered by Hon. F. J. Matthews; the *Valedictory* by Prof. S. M. Smith.

The names of the graduates will appear in our next No.

For receipts see cover.

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November, 1858.

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Anatomy—Microscopical Anatomy and Physiology.

J. W. HAMILTON, M. D.,
Surgery—Eye and Ear Surgery.

S. LOVING, M. D.,
Materia, Medica—Therap. Hygeine.

THEO. G. WORMLEY, M. D.,
Chemistry—Toxicology.

R. N. BARR, M. D.,
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